3

PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

SIXTH MEETING,

HELD AT ALBANY (N. Y.), AUGUST 1851.

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1852.

EDITED BY

SPENCER F. BAIRD,

Permanent Secretary.

And printed in Albany under the supervision of the following Committee, appointed for that purpose.

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Prof. J. Henry.

Committee to memorialize the Legislature of Ohio on the subject of a Geological Exploration of that State.

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S. Medary, Esq., Columbus.	John P. Foote, Esq., Cincinnati.
Judge Vance, Hamilton.	Hon. Allen Trimble, Highland Co., Ohio.
John H. James, Esq., Urbana.	

Hon. S. J. Andrews, Cleveland, Ohio.

Committee to memorialize the Legislature of Missouri on the subject of a Geological Survey of that State.

Prof. Silliman, Sen.	Dr. George Engelmann.
Dr. S. G. Morton.	Dr. H. King.
Prof. A. D. Bache.	Robert Buchanan, Esq.
Prof. Joseph Henry.	Prof. James Hall.
Prof. L. Agassiz.	Maj. M. L. Clark.

*Committee for memorializing Congress in relation to Scientific Explorations, and the distribution of the Duplicates from the Collection of the Exploring Expedition.**

Dr. Robert Hare, of Philadel'a.	Wm. C. Redfield, Esq., of N. Y.
Prof. Benj. Silliman, Sen., of N. Haven.	Prof. Benjamin Peirce, of Cambridge, Mass.
Prof. Stephen Alexander, of Princeton.	Dr. Robert W. Gibbes, of Columbia, S. C.
Prof. Henry D. Rogers, of Boston.	Prof. Louis Agassiz, of Cambridge.
Pres. Edward Hitchcock, of Amherst, Mass.	Dr. Samuel G. Morton, of Philadelphia.

* This committee has also been requested to consider the propriety of memorializing Congress on the subject of granting public lands to Missouri, for the purpose of carrying on a geological survey of that State.

Committee to arrange the detail of a System of combined Meteorological Observations for North America.

Prof. A. D. Bache.	Dr. T. R. Beck.
William C. Redfield.	Prof. A. Caswell.
Prof. Joseph Henry.	Hon. William Mitchell.
Capt. J. H. Lefroy.	Prof. Elias Loomis.
Prof. A. Guyot.	Prof. J. H. Coffin.

Committee to digest a plan of reducing the observations made under the direction of the Regents of the University of New-York, from 1825 *to* 1850, *with reference to their publication, and to decide upon the stations which shall be included in this reduction.*

Dr. T. R. Beck. | Prof. A. Guyot. | Prof. E. Loomis.

Committee to memorialize the State of New-York, and other States, in regard to geographical surveys.

Prof. A. D. Bache.	Prof. W. M. Gillespie.
Prof. O. M. Mitchel.	Samuel B. Ruggles, Esq.
Prof. E. Loomis.	Prof. C. Hackley.

Lt. E. B. Hunt.

Committee to examine Mr. Lyman's Telescope; the Stand for Mr. Simmons' Telescope; and the Mountain Barometer of Mr. Andrews.

Lt. M. F. Maury. | Prof. O. M. Mitchel. | Prof. A. Caswell.

Committee to examine the Microscope and Lenses submitted to the Association by Charles Spencer, Esq., *of Canastota, N. Y.*

Prof. J. W. Bailey.	Prof. J. Lawrence Smith.
Dr. J. Torrey.	Dr. W. J. Burnett.

Dr. Clark.

Committee to Memorialize Congress for an appropriation to enable Prof. MITCHEL *to perfect and apply his new astronomical apparatus.*

Prof. B. PEIRCE.
Prof. S. ST. JOHN.
Capt. CHARLES WILKES.
SEARS C. WALKER, Esq.
Prof. J. H. C. COFFIN.
Prof. A. D. BACHE.
Prof. W. B. ROGERS.
Prof. E. LOOMIS.
Lt. M. F. MAURY.
Prof. JOSEPH HENRY.

Sub-Committee, in the name of the Standing Committee, to revise, alter, adopt, and publish the Rules of Organization, presented at the meeting of the Standing Committee, held on Monday, August 26.

Prof. A. D. BACHE. | Prof. S. F. BAIRD. | Prof. JOSEPH HENRY.

Sub-Committee to prepare and present to the Association a System of Scientific Ethics.

Prof. JOSEPH HENRY.

OFFICERS OF THE CLEVELAND MEETING.

Prof. B. PEIRCE, *President*.
Prof. S. F. BAIRD, *Permanent Secretary*.
Prof. J. D. DANA, *General Secretary*.
Dr. A. L. ELWYN, *Treasurer*.

STANDING COMMITTEE.

Prof. L. AGASSIZ.
Prof. W. B. ROGERS.
Prof. B. PEIRCE.
Prof. S. F. BAIRD.
Prof. J. D. DANA.
Dr. A. L. ELWYN.

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Prof. S. ST. JOHN, *Secretary*.

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JOHN M. WOOLSEY, Esq.
H. C. KINGSLEY, Esq.
HORACE PERRY, Esq.
Dr. J. S. NEWBERRY.
Prof. H. L. SMITH.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

	Place.	Date.	President.	General Secretary.	Perman't Sec'y.	Treasurer.
1st Meeting,	Philadelphia, Pa.	September 20, 1848,	W. C. Redfield, Esq.,	Prof. Walter R. Johnson,	....	Prof. J. Wyman.
2d "	Cambridge, Mass.	August 14, 1849,	Prof. Joseph Henry,	Prof. E. N. Horsford, ...	....	Dr. A. L. Elwyn.
3d "	Charleston, S. C.	March 12, 1850,	Prof. A. D. Bache,*.	Prof. L. R. Gibbes,*....	....	Dr. St. J. Ravenel.*
4th "	New-Haven, Ct..	August 19, 1850,	Prof. A. D. Bache,..	E. C. Herrick, Esq., ...	....	Dr. A. L. Elwyn.
5th "	Cincinnati, Ohio.	May 5, 1851,	Prof. A. D. Bache,..	Prof. W. B. Rogers, ...	Prof. S. F. Baird,	Dr. A. L. Elwyn.
6th "	Albany, N. Y.	August 19, 1851,	Prof. L. Agassiz, ...	Prof. W. B. Rogers, ...	Prof. S. F. Baird,	Dr. A. L. Elwyn.

* In the absence of the regular officer.

CONSTITUTION OF THE ASSOCIATION.

OBJECTS.

The Society shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction to scientific research in our country; and to procure for the labors of scientific men, increased facilities and a wider usefulness.

RULES.

MEMBERS.

Rule 1. Those persons whose names have been already enrolled in the published proceedings of the Association, and all those who have been invited to attend the meetings, shall be considered members, on subscribing to these rules.

Rule 2. Members of scientific societies, or learned bodies, having in view any of the objects of this Society, and publishing transactions, shall likewise be considered members, on subscribing to these rules.

Rule 3. Collegiate professors of natural history, physics, chemistry, mathematics, and political economy, and of the theoretical and applied sciences generally : also, civil engineers and architects who have been employed in the construction or superintendence of public works, may become members, on subscribing to these rules.

Rule 4. Persons not embraced in the above provisions, may become members of the Association, upon nomination by the standing committee, and by a majority of the members present.

OFFICERS.

Rule 5. The officers of the Association shall be a president, secretary, and a treasurer; who shall be elected at each annual meeting, for the meeting of the ensuing year.

MEETINGS.

Rule 6. The Association shall meet annually, for one week or longer, the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be entrusted to the officers and the local committee.

STANDING COMMITTEE.

Rule 7. There shall be a standing committee, to consist of the president, secretary, and treasurer of the Association; the officers of the preceding year; the chairmen and secretaries of the Sections, after these shall have been organized; and six other members present, who shall have attended any of the previous meetings : to be elected by ballot.

Rule 8. The committee, whose duty it shall be to manage the general business of the Association, shall sit during the meeting, and at any time in the interval between it and the next meeting, as the interests of the Association may require. It shall also be the duty of this committee to nominate the general officers of the Association for the following year, and persons for admission to membership.

SECTIONS.

Rule 9. The standing committee shall organize the Society into sections, permitting the number and scope of these sections to vary in conformity to the wishes and the scientific business of the Association.

Rule 10. It shall be the duty of the standing committee, if, at any time, two or more sections, induced by a deficiency of scientific communications, or by other reasons, request to be united into one; or if at any time a single section, overloaded with business, asks to be subdivided, to effect the change, and generally to readjust the subdivisions of the Association, whenever, upon due representation, it promises to expedite the proceedings, and advance the purposes of the meeting.

SECTIONAL COMMITTEES AND OFFICERS.

Rule 11. Each Section shall appoint its own chairman and secretary of the meeting; and it shall likewise have a standing committee, of such size as the Section may prefer. The secretaries of the sections may appoint assistants, whenever, in the discharge of their duties, it becomes expedient.

RULE 12. It shall be the duty of the standing committee of each Section, assisted by the chairman, to arrange and direct the proceedings in their Section, to ascertain what written and oral communications are offered, and, for the better forwarding the business, to assign the order in which these communications shall appear, and the amount of time which each shall occupy; and it shall be the duty of the chairman to enforce these decisions of the committee.

These sectional committees shall likewise recommend subjects for systematic investigation, by members willing to undertake the researches, and present their results at the next annual meeting.

The committees shall likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent annual meetings.

REPORTS OF PROCEEDINGS.

RULE 13. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 14. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declares such to be his wish before presenting it to the Society.

GENERAL AND EVENING MEETINGS.

RULE 15. At least three evenings of the week shall be reserved for general meetings of the Association, and the standing committee shall appoint these and any other general meetings which the objects and interests of the Association may call for.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections; and thus all the Sections may, for a longer or shorter time, reunite themselves to hear and consider any communications, or transact any business.

It shall be a part of the business of these General Meetings, to receive the Address of the President of the last Annual Meeting; to hear such reports on scientific subjects, as, from their general importance and interest, the standing committee shall select: also to receive from the chairmen of the Sections, abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 16. The Association shall be organized by the President of the preceding Annual Meeting. The question of the most eligible distribution of the Society into sections shall then occupy the attention of the Association; when, a sufficient expression of opinion being procured, the meeting may adjourn; and the standing committee shall immediately proceed to divide the Association into sections, and to allot to the sections their general places of meeting. The Sections may then organize by electing their officers, and proceed to transact scientific and other business.

LOCAL COMMITTEE.

RULE 17. The standing committee shall appoint a local committee from among members residing at or near the place of meeting for the ensuing year; and it shall be the duty of the local committee, assisted by the officers, to make arrangements for the meeting.

SUBSCRIPTIONS.

RULE 18. The amount of the annual subscription of each member of the Association shall be two dollars; and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. The members attending an annual meeting shall pay, on registering their names, an additional assessment of ——— dollars. These subscriptions to be received by the treasurer or secretary.

RULE 19. The names of all persons two years and more in arrears for annual dues, shall be erased from the list of members; provided that two notices of indebtedness, at an interval of at least three months, shall have previously been given.

ACCOUNTS.

RULE 20. The accounts of the Association shall be audited annually, by auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

RULE 21. No article of this Constitution shall be altered or amended, without the concurrence of three-fourths of the members present, nor unless notice of the proposed amendment or alteration shall have been given at the preceding annual meeting.

RESOLUTIONS AND ENACTMENTS

OF A PERMANENT AND PROSPECTIVE CHARACTER,

PASSED PREVIOUS TO THE SIXTH MEETING.

Resolved, That copies or abstracts of all communications made, either to the General Association or to the sections, must be furnished by the authors; otherwise only the titles shall appear in the published proceedings. (*Proceedings First Meeting,* 1848, *p.* 133.)

Resolved, That 1000 copies of the proceedings of the Association be published in pamphlet form, and placed at the disposal of the chairman of the committee on publication.

(*Proceedings First Meeting,* 1848, *p.* 133.)

Resolved, That a *manual* or *manuals of scientific observation and research,* especially adapted to the use of the American inquirer, comprising directions for properly observing phenomena in every department of physical science, and for making collections in natural history, etc., whether on land or at sea, is much needed at the present time; and that such a publication, placed in the hands of officers of the army and navy, would greatly tend to develop the natural resources of our extended country, and to the general advancement of science.

Resolved, That the American Association for the Advancement of Science cordially recommends the Smithsonian Institution to undertake the preparation of such a volume, under the editorial superintendence of its Secretary, to be published in its series of reports.

Resolved, That this Association will cordially coöperate in the production of such a manual or manuals, in whatever manner may be best adapted to secure the end in view.

(*Proceedings Second Meeting,* 1849, *pp.* 273, 351.)

Resolved, That no paper be read before the future meetings of the Association, unless an abstract of it has previously been presented to the Secretary.

Resolved, That hereafter all books, charts, maps, and specimens, which may be presented to the Association, shall be given to the Smithsonian Institution. (*Proceedings Second Meeting*, 1849, *p.* 272.)

Resolved, That a secretary be appointed, who shall hold his office for the term of three years, and shall have a salary of $300 per annum, and whose duty it shall be to compile for publication all proceedings or transactions of the Association, to superintend the publication of the same, and to conduct the correspondence; the title of said officer to be that of Permanent Secretary of the American Association. (*Proceedings Fourth Meeting*, 1850, *p.* 16.)

Resolved, That the standing committee have power to fix the duties of the Permanent Secretary of this Association.

Resolved, That the Permanent Secretary be a member, *ex officio*, of the standing committee.

Resolved, That the Permanent Secretary be instructed to erase from the list of members of this Association, the names of all, who, by the return of the treasurer, shall appear to be two years in arrears for annual dues; suitable notice being given by two circulars from the treasurer, at an interval of three months, to all who may fall within the intent of this resolution.

Resolved, That the standing committee have full power to complete and finish any outstanding business of the Association, in their name. (*Proceedings Fourth Meeting*, 1850, *p.* 341.)

Resolved, That a copy of the printed volume of Proceedings of the Meetings at Philadelphia, Cambridge, and New-Haven, be presented to the libraries of Harvard and Yale.

(*Proceedings Fourth Meeting*, 1850, *p.* 346.)

Resolved, That the Treasurer be requested to retain $300 of the funds in his hands, and belonging to the Association, for the purpose of paying the salary of the Permanent Secretary: said payment to be made at such time, and in such manner, as may be agreed upon by the Treasurer and Permanent Secretary.

Resolved, That copies of the Proceedings of the American Association be presented to the New-York Lyceum, and the Philadelphia Academy of Natural Sciences.

Resolved, That the Permanent Secretary be requested to provide minute books, suitably ruled, for the list of members and titles of papers, minutes of the general and sectional meetings, and for the other purposes indicated in the rules.

Resolved, That whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the proceedings of the Association, that he be authorized to commit the same to the author, or to the proper sub-committee of the standing committee, for correction. (*Proceedings Fourth Meeting,* 1850, *pp.* 390, 391.)

Resolved, That copies of the Proceedings of the American Association for the Advancement of Science be presented to the American Academy of Arts and Sciences, Boston; to the Boston Society of Natural History, to the New-York Lyceum of Natural History, to the American Philosophical Society and to the Academy of Natural Sciences of Philadelphia, to the Smithsonian Institution, and to the Western Academy of Natural Sciences at Cincinnati.

(*Proceedings Fifth Meeting,* 1851, *p.* 249.)

[For the acts of the Sixth Meeting, see page 402.]

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

NOTE. Names of deceased members are marked with an asterisk (*); and those of members, who, in 1840, formed the original "Association of American Geologists," are in small capitals. The figure at the end of each name refers to the meeting at which the election took place.

A.

Abbott, Dr. S. L., Boston[1].
Abert, Col. J. J., Washington, D.C.[1].
Adams, Prof. C. B., Amherst, Massachusetts[1].
Adams, Solomon, Esq., New-York[1].
Agassiz, Prof. Louis, Cambridge, Massachusetts[1].
Aiken, Hon. William, Charleston, South-Carolina[3].
Alexander, Dr. R. C., Bath, England[2].
Alexander, Prof. Stephen, Princeton, New-Jersey[1].
Alger, Francis, Esq., Boston[1].
Allen, Prof. E. A. H., Troy, New-York[6].
Allen, George N., Oberlin, Ohio[5].
Allen, J. Burnet, Esq., Springfield, Massachusetts[2].
Allen, John H., Oxford, Maryland[6].
Allen, Z., Esq., Providence, Rhode-Island[1].
Allston, R. F. W., Esq., Georgetown, South-Carolina[3].
*Ames, M. P., Esq., Springfield, Massachusetts[1].
Anderson, Dr. Henry J., NewYork[4].

B

Andrews, Prof. J. W., Marietta, Ohio[5].
Andrews, Dr., Charlotte, North-Carolina[3].
Andrews, Joel W., Esq., Albany[6]. .
Anthony, Charles H., Esq., Albany[6].
Anthony, J. G., Esq., Cincinnati, Ohio[1].
Antisell, Dr. Thomas, New-York[2].
Appleton, Nathan, Esq., Boston[1].
Armsby, Prof. James H., Albany[6].
Ashmead, Samuel, Esq., Philadelphia[1].
Atkinson, Mr., Alexandria, Virginia[1].
Avery, Prof. Charles, Schenectady, New-York[6].
Ayres, William O., Esq., Boston[1].

B.

Bache, Prof. Alexander D., Washington, D.C.[1].
Bache, Dr. Franklin, Philadelphia[1].
Bachman, Dr. John, Charleston[1].
Bacon, Pres., Washington, D.C.[1].
Bacon, Dr. John, junior, Boston[1].
Bagg, Moses M., Clinton, New-York[4].
Bailey, Prof. J. W., West-Point, New-York[1].
Baird, Rev. E. Thompson, Washington College, E. Tenn.[6].
Baird, Prof. S. F., Washington, D.C.[1].
Baird, Thomas D., Esq., Baltimore[6].
Bakewell, Robert, Esq., New-Haven, Connecticut[1].
Bannister, Rev. Henry, Cazenovia, New-York[6].
Barbee, Dr. W. T., Jackson, Mississippi[5].
Barker, Dr. Sanford, Charleston[2].
Barnard, Henry, Esq., New-Haven, Connecticut[4].
Barnes, Capt. James, Springfield, Massachusetts[5].
Barratt, Dr. J. P., Barrattsville, South-Carolina[3].
Barton, William, Esq., Troy, New-York[6].
Beadle, Dr. Edward L., New-York[1].
Beardsley, R. G., Esq., Albany, New-York[6].
Beck, Dr. C. F., Philadelphia[1].
BECK, Prof. LEWIS C., New-Brunswick, New-Jersey[1].
Beck, Dr. T. Romeyn, Albany, New-York[1].
Belknap, George, Esq., Boston[1].
Belknap, Henry, Esq., Boston[2].
Bellows, Rev. H. W., New-York[5].

Beman, Rev. Dr., Troy, New-York[6].
Benedict, Prof. Farrand N., Burlington, Vermont[1].
Bigelow, Dr. Henry J., Boston[1].
Bigelow, Dr. Jacob, Boston[2].
*Binney, Dr. Amos, Boston[1].
Binney, John, Esq., Boston[3].
Blake, Eli W., Esq., New-Haven, Connecticut[1].
Blake, William P., Esq., New-York[2].
Blanding, Dr. William, Philadelphia[1].
Blaney, Prof. James V. Z., M.D., Chicago, Illinois[5].
Blatchford, Rev. Thomas W., Troy, New-York[6].
Bliss, George, junior, Esq., Springfield, Massachusetts[3].
Blumenthal, Prof. C. E., Carlisle, Pennsylvania[5].
Booth, Prof. James C., Esq., Philadelphia[1].
Bolton, Richard, Esq., Pontotoc, Mississippi[1].
*Bomford, Col. George, Washington, D.C.[1].
Bond, George P., Esq., Cambridge, Massachusetts[2].
Bond, Dr. Henry, Philadelphia[1].
Bond, William C., Esq., Cambridge, Massachusetts[2].
Bonnycastle, Sir Charles, Montreal, Canada[1].
Boutelle, C. O., Esq., Coast Survey[3].
Bouve, Thomas T., Esq., Boston[1].
Bowditch, Henry J., M.D., Boston[2].
Bowman, Capt. A. H., U.S.A., Charleston[2].
Boyd, Dr. George W., Charlottesville, Virginia[1].
Boyden, Uriah A., Esq., Boston[2].
Boynton, John F., Esq., Syracuse, New-York[4].
Brace, J. P., Hartford, Connecticut[2].
Bradley, Francis G., Esq., New-Haven, Connecticut[4].
Brainerd, Prof. Jehu, Cleveland, Ohio[5].
Braw, J. B., Esq., Hartford, Connecticut[5].
Breckenridge, Rev. Dr. Robert T., Lexington, Kentucky[5].
Brevoort, J. Carson, Esq., Brooklyn, New-York[1].
Brewer, W. H., Esq., Ithaca, New-York[4].
Briggs, C., Esq., Ironton, Ohio[6].
Brinsmade, Dr. Thomas C., Troy, New-York[6].
Brocklesby, Prof. John, Hartford, Connecticut[4].
Brown, Andrew, Esq., Natchez, Mississippi[1].
Brown, Richard, Esq., Sydney, Cape Breton[1].
Browne, Peter A., Esq., Philadelphia[1].

Brownne, Robert H., Esq., New-York[1].
Brumby, Prof. Richard T., Tuscaloosa, Alabama[1].
Brush, George J., New-Haven, Connecticut[4].
Buchanan, Dr. Joseph R., Cincinnati, Ohio[1].
Buchanan, Robert, Esq., Cincinnati, Ohio[2].
Buckley, S. B., Esq., New-York[1].
Buell, David, junior, Troy, New-York[6].
Bunce, John B., Esq., New-Haven, Connecticut[4].
Burden, Dr. T. L., Charleston[3].
Burgess, Rev. E., Ahmednuggur, India[1].
Burnet, Hon. Jacob, Cincinnati, Ohio[4].
Burnett, Waldo J., Esq., Boston[1].
Burr, E. T., Esq., Lyme, Connecticut[6].
Burton, Dr. C. V. W., Lansingburgh, New-York[6].
Burwell, Dr. George N., Buffalo, New-York[6].

C.

Cabell, Prof. James L., University of Virginia[6].
Cabot, Edward C., Esq., Boston[1].
Cain, Dr. D. J. C., Charleston[3].
Caldwell, John D., Esq., Cincinnati, Ohio[5].
Campbell, Robert, Esq., Pittsfield, Massachusetts[1].
Cappy, Dr. J. P., Montgomery, Alabama[4].
Carley, S. T., Esq., Cincinnati, Ohio[5].
Carpenter, George W., Esq., Albany, New-York[6].
*Carpenter, Dr. William M., New-Orleans[1].
Carter, R. C., Esq., Cincinnati, Ohio[5].
Case, Hon. William, Cleveland, Ohio[6].
Casseday, L. Addison, Esq., Louisville, Kentucky[5].
Caswell, Prof. Alexis, Providence, Rhode-Island[2].
Channing, William F., Esq., Boston[2].
Chapman, Dr. N., Philadelphia[1].
Chase, Prof. George J., Providence, Rhode-Island[1].
*Chase, Prof. S., Dartmouth, New-Hampshire[2].
Chauvenet, C. U., Esq., Annapolis, Maryland[2].
Chauvenet, Prof. William, Annapolis, Maryland[1].
Cheeseborough, A., Esq., Springfield, Massachusetts[5].
Chenoweth, B. D., Esq., Winchester, Virginia[4].
Chesbrough, E. S., West-Newton, Massachusetts[2].
Cheves, Dr. C. M., Charleston[3].

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Notices of errors or omissions in the preceding lists should be communicated to the Permanent Secretary.

ADDRESS

OF

PROFESSOR A. D. BACHE,

PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE YEAR 1851,

ON RETIRING FROM THE DUTIES OF PRESIDENT.

On retiring from the office of President of the American Association for the Advancement of Science, I submit, in conformity with usage, to its members, a few remarks in relation to the circumstances attending its organization, and to its progress, and some considerations of the direction in which we may look for its greatest usefulness.

The condition of society and of science of the day seems to have called for the organization of general associations for the promotion and advancement of science in nearly every country where its cultivators are numerous, zealous, and not closely gathered in one community; the precursors of more general unions for the same good purpose. To render such meetings practicable, modern facilities of communication are indispensable; and when these shall have brought Berlin and New-York as near as were Berlin and Paris at the close of the last century, we may pass from our present organization to something characteristic of the day of railroads and the dawn of telegraphs.

As the want seems to have been universally recognized, so it has been modified essentially by circumstances. In Germany, the cultivators of science have met in a social way, communicated, and dispersed. In Great Britain, an imposing permanent organization has kept the British Association always active, even when not together. Our own Association has scarcely developed a decided track: its end, the advancement of science; but the road to that end, left to the results of reconnoissance widely made on each side of the beaten path, to explore new ways around or through the obstacles.

Such associations cannot stimulate into being a Newton or a Cuvier; but who can say how much more they would have enabled Newton and Cuvier to do, by removing the load of opposition to their discoveries, by bringing up the cultivators of science as a body at once to the level of their knowledge, and by causing many minor lights to shine for the benefit of the time, either by reflection, or by the enkindling of new flame from their influence? Who will say that they do not return wiser, better, more zealous according to knowledge, from a meeting with Arago, with Humboldt, with Gauss, with Brewster, with Faraday, and their compeers; or, to come nearer home, with Henry, Peirce, or Agassiz? A man must be beyond improvement, indeed, whom such companionship will not benefit.

But is it true that genius is beyond or above the stimulus of association? Let the man among us who has, if ever man had the true "divine breath," tell us, in simple and single-heartedness, whether he left *that* meeting of the British Association the same man who went there; whether the effect of that simple and single figure on the blackboard, which showed to the geologists of the day discoveries to be made, founded on principles which created a new era in classification, was limited to his auditors, or even to cultivators of science through whom they spread with lightning rapidity and vividness: did it not react on him?

If such associations bring out only common men, it is because there are none others within their sphere of influence. Men of genius are still emphatically men, and of all others susceptible most truly of human and humanizing influences. Of them emphatically it may be said, if they will not say it for themselves, *Homo sum, et nil humanum alienum me puto.* The world is made up of ordinary men, and it is the part of common sense not to despise their doings. The specimens collected or the observations made by the humblest geologist who ever wielded a hammer, or the meekest astronomer who ever noted a transit, serve as part of the foundation of the superb structure raised by Von Buch, or by Leverrier. If the zeal of second-rate men is warmed into activity and directed in its development by such influences, the general level of science is raised by slow deposit, which may on occasion make mountains by upheaval. Associations are not proposed as a panacea, but only in cases needing a moderate stimulant: they appeal to some of the strongest and best motives of our nature.

Let us now briefly and rapidly glance at the general condition of

science among us prior to and at the time of the formation of our Association, the obstacles which were presented to its organization, and the mode of its formation.

In the colonial period of our country, the professors of mathematics and natural philosophy corresponded with the leading scientific men of Great Britain, some of them intimately : they looked for assistance in their pursuits (chiefly those of astronomy) to them, and for direction when special occasions of interest rendered their co-operation desirable. Winthrop of Massachusetts, and Rittenhouse of Pennsylvania, had the full advantage of such communion. At a later day, Franklin, Canton and Priestley, were intimates, and corresponded familiarly.

The generation which grew up during our revolutionary struggle, and after our independence was acknowledged, naturally did not succeed to these connections or friendships. The prosecution of mathematics and physical science was neglected; indeed barely kept alive by the calls for boundary and land surveys of the more extended class, by the exertions necessary in the lecture room, or by isolated volunteer efforts. As the country was explored and settled, the unworked mine of natural history was laid open, and the attention of almost all the cultivators of science was turned towards the development of its riches. Descriptive natural history is the pursuit which emphatically marks that period. As its exponent, may be taken the admirable descriptive mineralogy of Cleveland, which seemed to fill the measure of that day, and be as it were its chief embodiment, appearing just as the era was passing away.

I do not propose to attempt tracing the influences which have turned attention in America to a wider and deeper pursuit of natural, physical, and mathematical science. What we are here, at any time, lies more in present circumstances than in past history; and we share the general movement of the time, without those strongly conservative powers which in other countries exist in institutions of science and learning of a past day. The calls for mechanical knowledge, and for the applications of physics, of mathematics, and of natural science, have, without a doubt, thrown us irresistibly into the career which we are now following, and which, in its objects, aims and results, partakes of the general direction of the science of the world. The beginning of this movement was well nigh stifled by empiricism in forms, and in a strength which threatened the very life of science. Emboldened by the absence of accredited tribunals to

try its claims, it proffered boldly its pretensions to public notice, calling itself by the respected name of science, and to outward seeming entitled to its use.

In a small country town of France, as the worshippers were pouring from the cathedral church, I saw drawn up on the public square on which the building fronted, a large barouche, transformed for the time into a stage, from which a man, in a dress imitating that of the court of the last century, invited the issuing worshippers to try his skill in pharmacy, in medicine and in surgery, while a trumpet sounded occasionally appropriate "alarums" to call attention to the master charlatan. This was the old-fashioned character, hardly deceiving any but the most ignorant, though, withal, exhibiting a power of tooth-drawing such as would have challenged admiration had it been real. Our charlatans carefully doffed the dress, and laid aside the tools and stage, and their trumpet was blown by the spectators. They pretended, nevertheless, like him of the village, to that which they did not know; and invited, like him, the examination of powers which they did not possess. Had this association originated at that time, they would have usurped its seats, and outvoted the devotees of science in the election of its officers. This picture may seem overcharged; but I appeal for the essential truth of its features to the fears, which cannot yet be forgotten, of those who shrunk for many years from an organization, lest with the form of science it should want its spirit. The strife, though not a public and avowed one, has not been the less strenuous; and if renewed from time to time, the ground gained by true science is too well occupied by defensive works to render any new attack of avail. Our real danger lies now from a modified charlatanism, which makes merit in one subject an excuse for asking authority in others, or in all; and, because it has made real progress in one branch of science, claims to be an arbiter in others. Sometimes this authority is thrust on men who, not having the force to enlighten those who press them as to their real claims, injure the cause they would fain promote, by being too impressible. Merit thus moulded assumes the form of the impressing body. Whether the authority be seized or accepted, it is unlawful; the usurpers wear the shoes and buckles, if not the whole costume. This form of pretension leads men to appeal to tribunals for the decision of scientific questions, which are in no way competent to consider them; or to appeal to the general public voice from the decisions of scientific men or scientific tribunals, in matters

which, as they only are in possession of the knowledge necessary to make a right decision, so they only can give one which is valid. In a country where every thing is free, and every one may obtain a hearing, notoriety is often dearly purchased by the sacrifice of some portion of real reputation. Let us firmly discountenance the wearing even of buckles. If, even, we would count reputation by votes alone, the voice of *one* man of science is sure to be followed by *many* votes from the general throng.

The absence of a minute subdivision in the pursuit of science, the prevalence of general lecturing on various branches, the cultivation of the literature of science rather than of science itself, has produced many of the evils under which American science has labored, and which are now passing away. You have so much ground to clear, said an intelligent foreigner, that you cannot give all your time to one garden spot. We, though still farmers, begin to garden. While a general knowledge of various branches of science is useful in developing even a single branch, it is still certain that subdivision is essential to advancement. An Admirable Crichton rather fixes attention on his *own* perfection, than perfects any art.

Lecturing and the pursuit of science have, with us, up to this time, been very closely connected. It has been necessary to teach, and generally to lecture, in order to obtain means to pursue research; and the advantage which results from investigation is even now not so clearly seen as it ought to be by those who direct our institutions: they have yet, in many cases, to learn that the real estimate of a professor's services is not always the number of his hours on the college roster. Lecturing is, of all the arts, one of the most easily acquired, at least by our countrymen : it is undoubtedly useful, and most agreeable, but should not be the object and end of a man's career. It is not necessary to found institutions especially for its encouragement; nor should the power to diffuse science in successful courses of lectures be considered as a substitute for exertion in its advancement. One of the best lecturers in the world, confessedly so, made as great a failure in his first attempt as there is on record; and no one could detect the germ of one of our most brilliant lecturers in the unpromising envelope presented at a first lecture.

I remember well the chilling effect produced upon me, when young, by the remark of one of our leading literary men, applied to a distinguished scientific writer, that he was not a "mere *dry* man of science." The remark was intended for advice, and I pondered

over it. Perhaps I did not despise dryness as I ought; for the observation was drawn out by my unwillingness to undertake a notice of the first volume of Bowditch's Mécanique Céleste, some ten days after it appeared. Delighted with the idea of having the rich stores of that incomparable volume placed within the reach of a reader of the differential and integral calculus, I thought it profaneness to pretend to have read it in so brief a time. The immortal work was noticed by a more rapid reader. It was not then, and is not now, the prevailing fault of our science to be dry; nor is dryness one of the tendencies of our Association. I have sometimes thought there was danger of the opposite.

There is nothing more marked in different countries than the difference in facility of expression. Nothing certainly struck me with more force than the contrast between the happy fluency with which the Irish men of science brought out their ideas, and the difficulty which marked the expression of thought by their brethren on the other side of the channel. Some of the most brilliant discussions which I heard were in the French Academy, where the absence of dryness certifies that dryness is not, as in bitter reflection I may have supposed, a test of soundness.

The world's philosopher, Humboldt, speaks of the "self complacent diffuseness" of Aristotle; and if the Stagirite could show it in writing, we may well pardon it in oral communications. Manner is sometimes the index of mental workings, but not always. Much self-reliance may exist under a modest exterior, as apparent forwardness of manner may coëxist with a modest opinion of one's self. Let us be tolerant, unless we see the buckles.

When the effort was first made to establish a general American Association for the promotion of science, it is certain that it met with considerable opposition. There were various reasons for this. From close communication with many who are now active members of the Association, I know why this fear prevailed over their hopes of the usefulness of such an institution. The opposition came not more from those who were habitually conservative, than from those who, being earnest in regard to the progress of science, are usually in favor of all progressive measures. It proceeded from no under-estimate of the strength which there was among the cultivators of science. Some of us had studied the workings of the British Association, and had been convinced of the absolute necessity for the attendance there from year to year of the men of the universities, to give a tone to the

proceedings; and were alarmed, perhaps, at the forays into the domain of science, which had there been witnessed in some of the less powerful sections, and even into the park of Section A itself. So far from having been trained in the same schools, we scarcely knew each other personally. How could we irregulars venture into conflict, when the files to our right and to our left were strangers to us, and when the cause might thus have suffered from the want of discipline of its volunteer support?

It was very prudently left for the geologists to begin the work. In looking back, I see no reason to regret that such counsels prevailed. The geological surveys making in several States rendered meetings of those engaged in them very necessary, for comparison, discussion, systematic effort; for counsel, aid, and mutual improvement. A classification, or the basis of one, was to be made; and only by discussion, in such a body, could it be formed. In that association, positive work was the test of consideration: to be heard, a man must have *done* something; and the more he had done, the more patiently he was listened to. Thus, far deeper, morally, than the comparative depths which they explore, the geologists laid the foundation of the American Association. The naturalists associated themselves with the nucleus thus afforded, and the association became one of geologists and naturalists. Chemistry occupied from the beginning a portion of the attention of the Association, in its necessary connection with geology; at first a small, then a more extended part. Meteorology, which the circumstances of our country have made necessarily one of the branches of physics most successfully pursued among us, assisted in the further development; and calling in the votaries of general physics and mathematics, the association was expanded to its present dimensions, and became the American Association for the Advancement of Science. May the care thus taken in gradually raising the edifice from a firm foundation, secure its long duration!

I propose now, though conscious that the discussion must be a very limited and imperfect one, to add a remark to what has already been said on the benefits of associations like our own, to discuss the special advantages of our meetings; pointing out, as well as I may, those directions most likely to lead to our object, and some which I think, however alluring, should not be followed. But first a few observations on the ordinary modes of promoting science; in connexion with which, I would throw out for your consideration some

reasons which induce me to believe that *an institution of science, supplementary to existing ones, is much needed in our country, to guide public action in reference to scientific matters.*

One of the modes apart from education, by which, by common consent, everywhere science has been promoted, has been by the organization of societies for holding meetings, and publishing transactions and proceedings. Local institutions of this sort exist in all parts of the civilized world, sometimes endowed by the government, sometimes by individuals, and sometimes supported by voluntary contributions. To affirm that these institutions are not useful, would be to contradict universal experience : to withdraw our support from them, because they had failed to do all the good desirable, would be utopian. The present condition of science in France is in a great degree due to its Institute, which took the place of a less effectively organized body, when the nation determined to be the immediate patron of science. The departments have their societies, and some, as those of Lille and Lyons, with considerable vitality. In Great Britain, there is no large town without its philosophical or natural history society ; and in all the more important cities, there are as many societies as prominent departments of scientific research. In London, the subdivision is still more minute, and some branches have more than one association devoted to their advancement. Science cannot, in its writings of research, appeal to the mass of general readers ; and must be furnished, by association or endowment, with even its means of publication. Applied science is profitable in a pecuniary sense ; but abstract science, on which the other hangs, is not remunerating. Yet how many applications flow from one principle ! The world would gain, in a very high ratio, by bestowing its rewards for principles, instead of for applications.

With us, two philosophical societies only have struck very deep and wide their roots : the American Philosophical Society of Philadelphia, and the American Academy of Boston ; and several societies for the encouragement of natural history have been permanently useful. Not one of these associations is well endowed. For our only endowed national institution (the Smithsonian), we are indebted to the liberality of a foreigner ; and had it fivefold its present endowment, it would not be able to meet the actual demands upon its funds for purposes embraced in what its learned Secretary has classed as its " active operations" for " the increase and diffusion of knowledge."

The Institute of France gives its members a moderate support, that the country may have the benefit of their labors. The other institutions afford means for the publication of researches, but not, usually, for making them; nor, except incidentally in the persons of their officers, do they support their members. The means furnished for educational purposes are those generally which enable the votaries of abstract science to live. Where there are richly endowed universities and colleges, governed by the academic body itself, the facilities thus afforded are so extended as to require few others. Where institutions depend mainly upon the fees of pupils for their support, or, being endowed, are governed by those who take narrow views of the labors of scientific men, the professors are so loaded down with labor that neither body nor mind is capable of effective research. How very many there are who want only time and means for research, to advance those departments in which they now merely impart the doings of others! Will not a more healthy tone of opinion arise in time on this subject, from our intercommunication, and the candid expression of temperate and mature opinions?

Some of our institutions, and prominently among them the Franklin Institute of Pennsylvania, have furnished means for experiments on important subjects, and enlisted their most zealous members in researches; but even here the laborers were without hire, though neither they nor their works were deemed unworthy of it. Some of these researches remain to this day unpublished, from the necessary withdrawal of the members to other spheres of active exertion requiring all their time and thought; and will, if they have not already, become obsolete by the progress of the branches to which they belong. Among the obstacles to the progress of science with us, must be reckoned, as one of the largest, the want of direct support for its cultivators as such.

It is, I believe, a common mistake, to associate the idea of academical institutions with monarchical institutions. We show in this, as in many other things, the prejudice of our descent. We have among us the two extremes of exaggerated nationality and of excessive imitation: let us modify each by the other, and be wise. A national institute is not necessary to Great Britain, with her rich and powerful universities. Republican France has cherished her Institute, seeking rather to extend than to curtail its proportions. One of the most ardent of republicans is its perpetual secretary — that setting sun, whose effulgence shows that it is merely passing below the horizon

to illuminate another sphere! Nor does the idea of a necessary connexion between centralization and an institution strike me as a valid one. Suppose an institute of which the members belong in turn to each of our widely scattered States, working at their places of residence, and reporting their results; meeting only at particular times, and for special purposes; engaged in researches self-directed, or desired by the body, called for by Congress or by the Executive, who furnish the means for the inquiries. The detail of such an organization could be marked out so as to secure efficiency without centralization, and constant labor with its appropriate results. The public treasury would be saved many times the support of such a council, by the sound advice which it would give in regard to the various projects which are constantly forced upon their notice, and in regard to which they are now compelled to decide without the knowledge which alone can ensure a wise conclusion. The men of science who are at the seat of government either constantly or temporarily, are too much occupied in the special work which belongs to their official occupations, to answer such a purpose; besides, the additional responsibility which, if they were called together, they must necessarily bear, would prove too great a burthen, considering the fervid zeal, and I might almost say fierceness, with which questions of interest are pursued, and the very extraordinary means resorted to to bring about a successful conclusion. If it were admissible that I should go into detail on this subject, I could prove the economy of a permanent consulting body like this. This is, however, a lower view than the saving of character by avoiding mistakes and misdirection of public encouragement, and by loss of opportunity of encouraging that which is really useful. I should subject the Association to some criticism if I unfolded this subject specifically, particularizing the errors here generally alluded to; and I abstain, merely remarking that the amount which would have been saved to one department of the government alone, from the application of the principle of the equality of action and reaction, would have supported such a council for twenty years, including the furnishing of means to show experimentally the applications of the principle to the case in question. Not only in new undertakings would the advice of such a body be most important, but they would be appealed to for information in regard to existing ones, and would prove most serviceable in advising in doubtful points.

Our country is making such rapid progress in material improve-

ment, that it is impossible for either the legislative or executive departments of our Government to avoid incidentally, if not directly, being involved in the decision of such questions. Without specification, it is easy to see that there are few applications of science which do not bear on the interests of commerce and navigation, naval or military concerns, the customs, the light-houses, the public lands, post-offices and post-roads, either directly or remotely. If all examination is refused, the good is confounded with the bad, and the Government may lose a most important advantage. If a decision is left to influence, or to imperfect knowledge, the worst consequences follow.

Such a body would supply a place not occupied by existing institutions, and which our own is, from its temporary and voluntary character, not able to supply.

Astronomy, chiefly at first from its connection with navigation, has been the science which all governments, our own inclusive, have selected to encourage; fostering thus one of the highest branches of theoretical science, on account of its practical applications. It may be truly said that we know more of the laws which govern the motions of the distant bodies of the universe, than we do of those which regulate the constitution of bodies around us. Would not the same results, or assuredly similar ones, flow from a systematic encouragement for a long period of any one branch of science? The experiment is certainly worth trying.

If meteorology could be encouraged with a world-wide patronage, like astronomy, what practical and theoretical results would not be derived? The results of even the partial effort made in behalf of magnetism and meteorology, is encouraging : brief as the term has been, the materials are gathered, or gathering, from which important conclusions are daily derived, and which await the master mind to weave into new "Principia," a new "Mécanique," or a new "Theoria."

Every man of genius seems, on setting out from the mental level where education and circumstances have placed him, to be capable of a certain amount of effort in his "journey to the stars," and no more. Even animal natures are educated to view railroads without fright, first, and then without emotion, even of curiosity. No professor of physics lives that studies his pupils, who has not been disappointed at some time, after the elaborate preparation of a new experiment, to find how coldly it was looked upon : it was new to him; but to

his class all was new, and the same level included the motion of a needle by the galvanic current, and the magnetism of oxygen. The next generation will start from the level of the steamboat, the railroad, the phototype, and the telegraph.

It has seemed abroad, and with us in the United States, that something more was wanting to keep up the healing motion in the waters of science, than was obtained from the existing institutions already alluded to; that without interfering with their useful labors, good was to be gained by bringing their members together in one general association, holding its meetings in different places, in part to give facilities for attendance to different persons in turn, and in part to stimulate local exertion by the influence, so important in social as well as chemical action, of *presence*. Are such associations destined to an enduring existence, or are they only to be temporary in their action? Is their animation to be life-long, or to be from time to time suspended? If the want which they supply is temporary, they will have spring-time, summer, and winter. If, having fulfilled their end, they pass into other forms of institutions better adapted to the wants of science, we will not regret their longer or shorter life, nor hold them less in veneration that they died. The good they may do, cannot be lost.

Separated by vast distances, scattered in larger or smaller communities, the daily avocations of men of science in the United States keep us asunder. Our small numbers at any one point produces all the bad influences of isolation. We feel cut off from the world of science, and sink discouraged on account of the isolation; or having a position in the community about us, we become content to enjoy this, and forget that we owe a duty to the world outside; that we ought to increase, as well as to diffuse; to labor, as well as to enjoy the labor of others. Our country asks for other things from us than this; and men of science of this day will, as in times past, labor for progress. We will hope to have "American methods" in the other branches of science, besides practical astronomy.

If these associations have proved themselves of value in other countries, and have commanded the support of all their most active and eminent men of science so as to continue their meetings year after year, there is none where they could have promised to be so important to the interests of national science as in the United States. Organization here, for good or for evil, is the means to the end. While science is without organization, it is without power: power-

less against its enemies, open or secret; powerless in the hands of false or injudicious friends. Not wedded to existing forms, this country is alive to everything which promises improvement; and the public mind, in this or that place, or in the whole country, made almost a physical point by the electric telegraph, runs irresistibly in one course, the result of wise or evil counsels, of knowledge or half-knowledge. Honor to that great statesman who, passing beyond the limits of our political and social institutions, came forward on our national anniversary to direct the minds of the people in tracing the progress of our country by its education, its religion, its literature, and its science!

Many of our States are anxiously alive to the promotion of science, both directly and indirectly; and it is of the greatest importance, that in moving, they should go rightly. The legislative and executive branches of our General Government are called upon often to decide questions which belong rather to scientific than to political tribunals. A timely recommendation by a scientific congress would frequently be a relief from serious embarrassment, and ensure the most beneficial results to the progress of science. The abuse of such power is less to be expected in this than in other bodies, because reacting immediately upon the body itself. Thus far no single recommendation made by the American Association has fallen powerless: they have both done positive good, and averted positive evil.

In looking at the labors of associations like our own, we naturally desire to emulate them; and the spirit of imitation is second nature. We are prone to think, that what is well done, and successfully, by others, we should prove our ability to do; and that omission is a confession of inability. On a warm and sunny afternoon, I saw the company in a railroad car prepare a shade before leaving the depot, by raising the blinds on the east side, because some one who had his head turned set the example. The value of the example which we would emulate or imitate, may consist in the circumstance—on the side where the sun is.

Let us take up some of the leading objects to which other associations have usefully devoted their energies, and see whether they constitute leading marks for our course.

One of the most useful fruits of associated scientific labor is in making, directing, or furnishing the means and appliances for experimental researches. There is a class of experiments and observations, the plan of which can be laid out beforehand, and which it is

eminently the province of associations to undertake. The British Association has distinguished itself by directing such through committees, and considerable appropriations have been made for their necessary expenses. The fee of membership in that association is large; the number of members, very large : we cannot expect to emulate it in our pecuniary means. The treasury of our Association has been relieved by the liberality of the city of Charleston, and of the citizens of Cincinnati, from the cost of publishing the proceedings of the meetings; and yet it is very scantily supplied. Can we collect means for directing researches, unless from unlooked-for individual munificence? I think not. Nor do I see that working spirit in our committees, which alone could bring experiments to a successful conclusion. The Association disperses; the members separate, and there is no stimulus to apply to committees for the half or the whole year. Even the physical constants we have called for, have not been reported.

There is one subject in this connection, which I feel it my duty briefly to advert to. Painful though it be, it should not be passed by. I do not see, on the part of the younger members of the Association, that disposition which belongs to their time of life, to take the laboring oar in the details of our own work, and without which even the temporary labors of our meetings cannot be long borne. Perhaps, from modesty, they shrink from the responsibility. If so, let them be encouraged to do their part, in confidence that the desire to serve will be fully appreciated by the Association.

I cannot see that experimental researches, undertaken from its funds and executed by its committees, can successfully form part of the regular business of this Association.

These remarks I do not at all apply to cases where, means being furnished by public or private munificence, the question is merely to direct a plan of operations. We can, assuredly, to advantage, direct the researches of others by suggesting what it is desirable to do, or how it should be done. If experiments on the change of level of our coast, on the sediments of rivers and estuaries and the like, are desired by geologists; if special collections are desired by our naturalists, and there are public works to which these matters appropriately belong, or private individuals who desire to see them carried through, this Association is a very proper body to suggest the observations and to furnish instructions.

The standing committee of the Association, and others organized

to act during the meetings, have always done much work. The members devote themselves to the Association sedulously during the days of meeting. Why not limit ourselves generally to those subjects and matters which our committees can transact during the period of meeting, and why not give time for committees to deliberate? Do we not press matters too much, for the interest of the Association? After close observation, I believe that we do. We ought to allow ourselves time to do well what we undertake.

The same course of reasoning would lead me to the same conclusions in regard to computations to be made under the direction of the Association; another field of usefulness, which the British Association has fully entered into, attaching its name to the most extended and best arranged catalogue of the stars which has yet been published.

If we would attempt reports on the progress of science like those which were so perseveringly and admirably kept up by the illustrious Secretary of the Swedish Academy, or like those which have emanated from the committees and members of the British Association, we are on preoccupied ground, with disadvantages of position and of pecuniary resources. Not only must we compete in our own language abroad, but with an institution at home (the Smithsonian), which, finding this field of usefulness untilled among us, has fully entered upon it. This career of usefulness is, except on special occasions, not open to us.

The objects of our Association are to be considered as they act directly to increase the amount of scientific knowledge, or indirectly by the effect on the associates who attend the meetings, or the communities where they are held.

Without a published record of our doings, the effect of our Association would be limited to the members who attended the meetings, and the importance of publishing has been recognized in the informal arrangements from year to year in reference to it. The zeal of the local committees at Charleston and Cincinnati not only relieved the Association from the expense of publishing the volumes, but carried them rapidly through the press. To the local secretary at Charleston, Prof. Lewis R. Gibbes, and the permanent secretary of the Association, Prof. Spencer F. Baird, we are indebted for the promptness with which the volumes of the Charleston and New-Haven meetings appeared. When the meetings are merely annual, there will be time, it is hoped, to permit authors carefully to examine

the proofsheets of their papers, without which dispatch is gained at the expense of accuracy.

It is hardly possible that the publication of our proceedings should interfere with the transactions of other learned societies. Our materials consist essentially of abstracts or of brief communications, of accounts of researches often in progress, and notes which differ entirely from the elaborate memoirs appropriate to such transactions. The memoirs of learned societies, with us, have always been published at considerable intervals of time; and I am not aware that the intervals have been increased since our organization. There is unmistakable evidence in the activity of the American Journal of Science, that we do not interfere with it.

At our meetings have been presented 338 communications; of which, 107 have been on subjects of physics and mathematics, 32 on chemistry, 93 on geology and mineralogy, 83 on natural history in its various branches (especially zoology), and 23 on miscellaneous subjects.

For these publications, I am of opinion that we have drawn in part from material which had accumulated : we have consumed more than the supply of the year would furnish, and will at last, when thrown upon the results of a single year, find our proceedings less abundantly supplied than hitherto. Still, while our gatherings are numerous as now, and similarly constituted, there will be interesting matter and to spare. Matters in progress will be brought before us, the full results of which will be published elsewhere.

But, have any papers or memoirs been actually produced by these meetings, which would not, without them, have been brought before the public? Of this I entertain not the least doubt. Indeed, I know many which, without the stimulus of preparation for these meetings, would not have seen the light; some, which, in fact, could not appriately have been brought forward elsewhere. The desire to add to the interest of the meetings, has been a powerful stimulus to exertion on the part of many. This will continue, perhaps, intermittingly; but, as localities change, and new members attend the meetings, the average may remain nearly constant. Papers have been produced, which otherwise might not have appeared. We are posted up to the very day of meeting in the researches actually making in natural, physical, and mathematical science. At the Cambridge and New-Haven meetings, physico-mathematics had the leading part; at Charleston, natural history, and especially zoology; at Cincinnati,

geology and its kindred branches. Each meeting was characterized by communications of a very high order of interest; each as distinctly characterized as the part of the country in which it was held; each one excellent in its way, exhibiting (like our union) unity in diversity.

For the good effect, from our meetings, on local science, I appeal unhesitatingly to those who have been with us since the new organization. We know that such good has resulted. We can point to those who have found new motives to research in attending our meetings. We can point to one institution, founded and endowed in part through our action on public sentiment. May we be able, also, at no distant day, to say of another which is an ornament to the last noble city where we assembled, that it too has been endowed in consequence, at least in part, of our influence.

There are some subjects which only an association like this is competent to grapple with: the subject of regulating standards of weights and measures, and the scales of barometer and thermometer, and the prime meridian, are of this kind. There is now no other scientific body to point out explorations desirable to be made on land or water, to suggest systems of extended meteorological observation, plans of surveys, geographical, geological, or others.

Congress, after changing the money standard and regulating the coinage, seems to have stopped short, and, after a long agitation of the subject of standard weights and measures, to have left it in very weariness. The whole effort towards uniformity has simply been to prepare material standards of a quality according with the demands of the science of the day: pounds, yards, bushels and gallons conforming to the general average of those in common use, and derived from England; and putting aside innovations in the commonly received units, their multiples or sub-multiples. I have been gratified to see a spirit of inquiry on this subject alive among the members; believing that nowhere can essential changes or reforms in the whole system be more fairly or powerfully discussed, than here. An individual, unless perhaps some leading legislator, who would propose changes at this time in our country on this subject, would but waste his time and logic.

The Association determined in Cincinnati to fix a time for the discussion of this subject at the present meeting. Closely connected with it is the regulation of the various scales used with instruments, which depend on the unit of length measure, or arbitrary scales. I

hope to see a time set apart at this or some subsequent meeting for the discussion of this matter, which was partly opened at Cambridge by Prof. Guyot. The world is obviously ripening for a general settlement of these questions, and as intercommunication strengthens the advantage of one system of measures and weights, and of one general prime meridian, will gain strength with it. A collection of the weights and measures, the barometers and thermometers, and of the charts of navigation and nautical almanacs of each of the countries represented at the World's Fair, would have presented in strong relief the necessity for something better adapted to a world's use.

As far as these questions affect scientific men and science in the United States alone, they are absolutely within our control; and the recommendation of this body would undoubtedly lead to the adoption by our brethren of such standards of weight and measure, such linear unit for the scale of the barometer, and such scales for the thermometer as would be recommended. Those who use these standards would be the first to become familiar with them from actual sight and use; and I know the great ease with which one becomes used to measures and weights, the employment of which seems at first entirely strange.

If ever we may expect a combined series of meteorological observations with exact instruments and observers, whose *business* it shall be to make the observations, it must be in consequence of a recommendation of this Association. The stations should be selected so as to furnish the best results for climate and the laws of storms, and not left to the determination of circumstances foreign to the consideration of the object sought. If we could once communicate to New-York harbor the coming of a northeast storm in time to prevent vessels from leaving it, the full benefit of these researches would come home to the commercial community of the country, and means to extend the observations would be freely provided.

The recommendations already made by the Association have met with signal success. Among these I may note the appropriation for the publication of the report of Prof. H. D. Rogers on the geological survey of Pennsylvania, by the legislature of that State; the law for a geological survey of Ohio; of the scientific explorers attached to the Mexican Boundary Survey, under Commissioner Bartlett; and of the expeditions under charge of Lieut. Maury, for examining special questions connected with winds and currents.

It has been doubted whether it was expedient for the Association to give opinions on questions of science, and to report on scientific researches. These doubts I do not share, but believe, on the contrary, that if we decline so to do, we do not come up to the clear measure of our duty. Where the opinion of the Association is sought, it should fearlessly, frankly, but carefully be given. The fact that the researches are by one of its members will not excuse it for avoiding the task. Instead of stating in the approved formulary that it gives no opinion, I would have it guide opinion by forming its own mature conclusions, and giving them where desired with all the weight of its authority.

Whether the Association should be the arbiter of questions of priority occurring between its members, is much more doubtful in my mind. It would be of great value to scientific men to have such questions, which unhappily often arise, carefully and dispassionately settled. When both parties desire the arbitrement, I think it should be assumed, as a less evil than that of avoiding a decision where the facts are all presented. There is but one other course : rigorously to exclude all personal questions ; in which case, the Association decides that there is no tribunal for such, and drives its members to appeal to the public.

At the last annual meeting, the president of the year before was called upon to prepare a code of scientific ethics, the result of the clear principles laid down upon this subject in his address. Let us hope that he will contribute this code, which, like reputable men under the civil code, we will endeavor to live without violating; not considering it a law given to compel us to right, but rather as a line far within which we will walk.

In considering these meetings in reference to the effect on the individual members, we might claim them as a relaxation from severe labor; as an agreeable, intellectual resort, to learn the results of the day, did not our aims soar higher than this, and extend to substantial intellectual benefits to others as well as to ourselves.

That these meetings are, in the individual intercourse which they bring about, intellectually and morally beneficial, is most certain. The intellect is excited by such intercourse; the heart is expanded. Freedom and frankness of discussion, and the interchange of views and friendly criticism, have marked the meetings. If there have been exceptions, the shock that was produced has recalled better feelings, or better judgment. It is certainly true, that as far as a man's re-

searches and discoveries are comprehensible in his day, he receives full credit for them, especially if he ask little and is patient. Contention about priority, or contention at all, is as a general rule unnecessary; where it becomes personal, it disgusts, and reacts against him who indulges in the personality. It is difficult to avoid controversy; but, if obviously forced on one, the defence finds itself strong in sympathy. We have not been without examples of the good effect of submitting to scientific discussion and decision, pretensions, the reality of which the author did not, and could not doubt, but which had been doubted by others. What triumph greater than that of our Cincinnati brother, when the committee of the Association reported so strongly in favor of the admirable method of recording right ascensions and declinations. Truth triumphed through his love for it! What a triumph for American science, when the "American method" of observing is adopted at Greenwich! The contribution, by whomsoever made, by however many shared, is a contribution to the glory of the country. The generous award of credit to our country, by this name, by the illustrious astronomer of Greenwich, is not the less honorable to him than to us. Let us show ourselves worthy of the spirit, by sinking all personal views in a general contribution to the American method. If the Association is worth any thing, it should be adequate to this result : let it be the test.

To our meetings each member brings his contribution; gathered from the land or the sea, the earth, the air, the heavens, the spirit, each one lays his offering on the altar of truth. How little, when under the influence of this spirit, seem contentions for special lines of research, the interference in special pursuits, the covetous desire to enter a particular path, and the determination to jostle the votary who is following it, rather than not to occupy it exclusively. The realm is boundless; the paths are numerous : each one is wide.

Let there be no contention, brother, between thee and me! Let there be rather a generous and eager urging forward, each of the other, to the good which we all seek. Warmed by the glow of generous sympathy, let us find our zeal kept alive by association, and show that the bond of scientific brotherhood is worthy of christian men, in a christian land, of the faith which we profess in time and of our hope in eternity.

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ALBANY MEETING, 1851.

COMMUNICATIONS.

A. MATHEMATICS AND PHYSICS.

I. MATHEMATICS.

1. On the Relation between the Square Roots of Negative Quantities, and the Principle of Perpendicularity in Geometry. By John Paterson, of Albany.

(1). Much discussion has been held by geometers on the value and significance of the positive and negative signs of algebra, and still more emphatically on the so-called symbol of imaginarity derived from the latter sign, and commonly expressed as the square root of a negative quantity; and although it has long been admitted, as a mere result of observation, that the product of like signs is positive and that of unlike signs is negative, and more recently that the sign of imaginarity coincides (fortuitously perhaps) with the principle of perpendicularity, these correspondences and agreements being sufficiently well determined and safe for all practical purposes; yet it is still a desideratum with those who desire the establishment of precise definitions at the commencement, as well as clear deductions in the progress, of a science confessedly and unerringly accurate in all its conclusions, that these accidental coincidences and loose analogies

should, as far as possible, be eliminated, and their place supplied by consecutive and logical sequences. In the faint hope of contributing something towards this desirable object, I have not long since printed a volume of researches in the calculus of operations; and it is now my wish to present a very brief account of the first part of those researches, that is, so far as they relate to the interpretation of the algebraical signs. In undertaking to do this, I have to apologize deeply for my unfortunate want of talent at exposition, and to protest against the notion of an undue abstruseness of subject. Abstruseness and incoherence of manner is a fault of my own; but I confidently apprehend that the method by which I interpret the elementary operations of algebra offers no greater degree of abstruseness or far-fetched abstraction, than does the beaten but inconsequent path hitherto followed. It has grown almost to be a stipulation of the age, that the writer whose sentences cannot be read as we run shall not be read at all; and in addition to this drawback, I must yet encounter the charge of impertinence, in attempting to controvert the authority of a great name, or it may be said of many great names, whose united or single competence to fix irrevocably the elements and definitions of a science it were madness to call in question. Such temerity on the part of an obscure individual unavoidably begets a suspicion of charlatanry, which attenuates very materially his chance of receiving a proper share of candid attention, and thus retards indefinitely the progress of his cause.

(2). Such, in my own estimation, is the simplicity of the calculus of operations, that its principles and method need but to be explained, in order to be understood and admitted. To use general terms at first, an *operator* performs an *operation* in space and time, according to a law of action and under conditions of space previously assigned; and the *calculator* must record *the measure* both of *the result* and of *the operation performed*. For instance, a porter carries a given burthen a given distance in a given time : the value of the burthen in its new place of deposit is the measure of *the result* of the operation or labor, and the distance it was carried is the measure of the labor or *operation* itself. As a second example, two ships land cargoes of equal weight and value at the port of New-York, one having traversed the ocean from Liverpool in England, the other from Canton in China : the measure of the result is the same for each, the equal values of the cargoes at their place of delivery; but the measures of the operations which produced these results, that is, of the distances

which the cargoes were carried by the ships, differ materially, and for certain purposes should be recorded as forming an essential consideration in the *complete* result. In the algebraical calculus, the measure of the result alone of the operation is recorded, that of the operation itself being generally overlooked or indistinctly understood. It is by taking note of the existence and value of this last measure, in operations performed under several different laws and conditions, that I have endeavored to collect the materials and principles, and lay the foundation of a new method, which falls under the designation of the calculus of operations.

(3). I cannot remain under the hypothesis which regards the successive states of the universe as happening by virtue of a sort of magician's presto, whereby the same body is made to appear in different places without passing through the intervening space. The poet, even, had some doubts as to the propriety of annihilating space and time : it should be the graver office of the mathematician to watch that such sacrifice be not wantonly performed; for, without the inviolability of the ineffable element of space, and its eternal concomitant time, we are truly a lost world, and truth can have no point of equilibrium. Yet what does the algebraist, when he inquires why the third term of Taylor's series should be rejected beside the second, but forget the very simple fact that a material body cannot be in two places at once?

(4). I propose, then, to pass from the counting of *things*, to the counting of *actions*, but of actions so defined that they are as accurately measurable as is any *thing* itself, and indeed by the very same operation. Space, whose nature can here be only treated dogmatically, is the element of all measurement : abstracted from its coeval element time, it measures simultaneous values; conjoined with time, it measures successive values. Space has three dimensions: linear, superficial, and solid; and the two last are algebraically expressed by well-known functions of the first. We are thus referred to the straight line as the indispensable and sufficient element for the admeasurement of all the things that are, and of the events that occur, or the operations that may be performed in space and time.

(5). Let our operation consist of a uniform movement in a plane. We recognize at first two different species of uniform movement, linear and angular; and each of these subdivides into two varieties, a direct and an inverse movement, accordingly as, in linear motion, we proceed from the origin towards the right hand, or towards the

left hand; and, in angular or circular motion, from the right hand upwards or downwards from the horizontal axis. The uniformity of movement is the law of action, and the particular direction of the motion is the condition of space, under which the operation in a given case is to be performed.

(6). The first and simplest form of mathematical operation is that of addition; which, being regarded as a *direct* operation, has of course its corresponding *inverse*, the operation of subtraction.

As a familiar illustration of the operation of addition, let it be proposed to add together two separate parcels (7 and 5) of material things or units of any description, for instance ivory balls such as are used to exemplify operations in the calculus of probabilities: the arithmetical result, or sum, is 12 balls; but by the calculus of operations, in which we are to seek the measure of the operation, we proceed as follows:

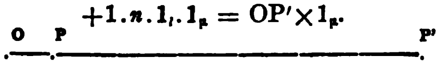
At A is a reservoir containing an indefinite number of balls, and at B is a receptacle containing 7 balls, and I am directed to add 5 balls to the 7 already contained in B. It is necessary to transfer 5 balls from A to B, and the distance AB (which may be called *one*) is the measure of the operation. It is the measure of the operation in *magnitude* or *quantity;* and if we wish to record also *the quality* of the operation, as being direct, cumulative or positive, the direction in the plane of the operation, from A towards B, or to the righthand of the point A, may be termed the positive direction, and the result written

$$(7+5) \text{ balls} = +12 \text{ balls};$$

the symbol $+$ denoting *the quality of positivity*, the geometrical signification of which is a conventional *direction* in space. Observe that the 7 balls in B at the beginning of the example were similarly obtained by transfer from A, and therefore the sign is correctly prefixed to the result $(+7+5) = +12$, the measure of the operation being implied by the sign. Now the labor of carrying 12 balls once through the distance AB, is in general equal to that of carrying one ball through 12 times the distance AB; that is, the measure of the operation is the same in these two cases. However obtained or generated, therefore, the abstract number 12 may be made the coefficient of any arbitrary unit of space, as for instance the unit of distance, which is at once the simplest and most general form of the measure of an operation; and when our abstract number is affected

with the positive sign, the quality or direction of the operation in space is fixed. Substituting general symbols, then, for the foregoing particular ones, I write $+1$ for the unit of positive direction, n for the abstract number, $1_{\prime}$ for the linear unit measure of the magnitude of the operation, and 1_{μ} for the material unit, or unit operated upon, and the complete concrete result of a simple direct or positive operation is expressed and represented by

$$+1.n.1_{\prime}.1_{\mu} = OP' \times 1_{\mu}.$$



The number n is therefore transferred from arithmetic to the calculus of operations, by prefixing to it the sign of direction, and suffixing the unit of distance; and the number or ratio n is now regarded as generated by the operation of transferring the material unit 1_{μ} from zero through the distance $n1_{\prime}$, in the time $n1_{t}$.

(7), The operation inverse to the one above considered, will consist in the subtraction, first, of 5 balls from the 12 balls contained in

the receptacle B; and as this operation is destructive of the effect of the addition of the 5 balls to the receptacle B from the reservoir A, it will be correctly performed by returning the 5 balls from B to A, in which the magnitude or *quantity* of the operation is the same as before, and its opposition in *quality* to the operation of addition is indicated by the opposite direction BA in which it is performed; and thus opposition of direction in space becomes identified with the symbol — (of subtraction) which denotes *the quality of negativity*, the result being expressed by $(+12-5)$ balls $= +7$ balls. A second operation may be the subtraction of the remaining 7 balls, which will be expressed by

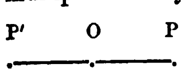
$(+12-5-7)$ balls $= (+7-7)$ balls, or

$(+12-12)$ balls $= 0$ balls, the result of the operation. In the righthand member of the last equation, the measures of the positive and negative operations are eliminated by mutual destruction, but both are in evidence in the lefthand member. Recalling the fact that the operation of transferring n material units through the unit of distance is equivalent to the transfer of 1 material unit through n units of distance, we may write the equation thus:

$$+12.1_{\prime}.1_{\mu} - 12.1_{\prime}.1_{\mu} = 0.1_{\prime}.1_{\mu}.$$

The first term of the lefthand member expresses the transfer of the material unit 1_μ from O to P$''$, where $OP'' = 12 \times OP' = +12.1_\iota$; and the second term expresses the return of 1_μ from P$''$ to O, in obedience to the geometrical signification of the sign —. Now the order of the performance of these two operations is indifferent, and the last might be the first; that is, from the origin O, the material unit 1_μ may first be transferred to $P_{\prime\prime}$ on the lefthand of O, where $OP_{\prime\prime} = 12 \times OP_{\prime} = -12.1_{\prime\prime}$, and thence returned by the term $+12.1_\iota$ from $P_{\prime\prime}$ to O its primitive position, giving the same result $0.1_\iota.1_\mu$ as before. In general symbols, then, the complete concrete result of a simple inverse or negative operation is expressed and represented by $-1.n.1_\iota.1_\mu = OP_{\prime\prime} \times 1_\mu$.

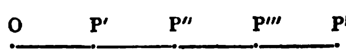
(8). The next form of mathematical operation to be considered is that of multiplication. Now when the multiplier is positive unity, the operation is identical with the performance of one operation of addition; and consequently if the multiplier be negative unity, the operation must be equivalent to the performance of one operation of subtraction as above interpreted. It is a postulate in geometry, that a straight line of any length may be drawn in any direction from any origin. This is *an operation;* and I ask no more than this, except the permission to record the line or distance described as the measure of the operation of describing it. Then let 1_μ be the multiplicand, and multiplication by positive and by negative unity will be respectively



represented by $OP \times 1_\mu = +1.1_\iota.1_\mu$ and $OP' \times 1_\mu = -1.1_\iota.1_\mu$; each operation consisting in the separate transfer of the material unit or multiplicand 1_μ, from the origin zero at O, through the unit of distance OP or OP$'$ in the unit of time 1_ι.

(9). I now omit the sign, and proceed to investigate the results and measures of repeated multiplications by positive unity. The multiplicand being always 1_μ, let the multiplier be

$1_\lambda = OP' = P'P'' = P''P''' = P'''P^{iv} = 1_\iota$ the linear unit, and the results will be



$$
\begin{aligned}
1_\lambda.1_\mu &= OP' \times 1_\mu = 1_\iota.1_\mu, \\
1_\lambda^2.1_\mu &= P'P'' \times OP' \times 1_\mu = OP'' \times 1_\mu = 2.1_\iota.1_\mu, \\
1_\lambda^3.1_\mu &= P''P''' \times OP'' \times 1_\mu = OP''' \times 1_\mu = 3.1_\iota.1_\mu, \\
1_\lambda^4.1_\mu &= P'''P^{iv} \times OP''' \times 1_\mu = OP^{iv} \times 1_\mu = 4.1_\iota.1_\mu, \text{ etc.}
\end{aligned}
$$

Thus, by direct operation, the involution of a linear unit to the *n*th power produces a linear unit equal in magnitude to *n* times the unit involved, or the power is *n* times the root in magnitude; but as the linear unit is absolutely indeterminate, the result of its involution is always an equally indeterminate linear unit, there being no distinctive quality whereby to recognize it; and we consequently have always the numerical equality $1^n = 2$, so long as the measure of the operation is not taken into the account. When this measure is brought in, we perceive that the process consists in nothing more than the successive addition of units of linear space; but to the suggestions derived from the contemplation of this simple process am I indebted for whatever of good or ill fortune shall betide me in these my perilous investigations.

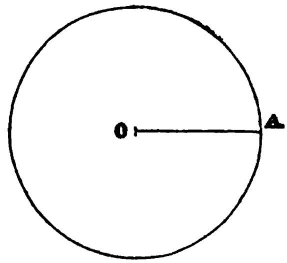
(10). From this first position in which the operations of multiplying by positive and negative unity *agree* entirely with the operations of adding and subtracting, we must advance a step to meet *the difference* which so remarkably distinguishes the former from the latter, and yet preserves the harmony between them.

While linear extent has no natural or fixed unit, and no limit but infinity, we have *an absolute geometrical unit* in the sum of four right angles, as measured by the circumference of a circle; a unity which may be repeated, but always with coincidence. The whole infinity of space is included by the four right angles about a point (if a plane is alone the subject of observation) or axis (when the three dimensions of extent are regarded): consequently, the radius being granted, any given magnitude of area may be measured by *one* operation of revolution; and a similar remark may be made with respect to a plane and volume.

(11). Instead of a linear, take now an angular multiplier; the angle, as customary, being measured by its subtending arc to radius unity. It is a postulate with Euclid, that a circle may be described on any centre and with any radius. This is *an operation;* and I demand no more than this, save the privilege of recording the arc described as the measure of the operation of describing it; while the radius, considered as revolved by the action of the describing compasses, shall in its final position be regarded as the measure of the result.

Let the multiplicand be the radius OA $= 1_r$, loaded with the material unit 1_μ at its extremity A; and take first for multiplier the entire circumference, denoted by 1_0. The operation consists in revolving one limb of the compasses, the radius accompanying, in the

positive angular direction once about the centre O, from the position OA to OA again; and this is multiplication by an absolute geometrical unit.



Since $OA \times 1_\mu = 1_r . 1_\mu$, one operation produces

$$AA \times OA \times 1_\mu = 1_0 . 1_r . 1_\mu ;$$

and continued repetitions yield the series (including also the first term),

$$\begin{aligned}
AA \times OA \times 1_\mu &= 1_0 . 1_r . 1_\mu, \\
AA \times OA \times 1_\mu &= 1_0^2 . 1_r . 1_\mu = 1_0 . 1_r . 1_\mu, \\
AA \times OA \times 1_\mu &= 1_0^3 . 1_r . 1_\mu = 1_0 . 1_r . 1_\mu, \\
AA \times OA \times 1_\mu &= 1_0^4 . 1_r . 1_\mu = 1_0 . 1_r . 1_\mu, \text{ etc.}
\end{aligned}$$

The second column shows the measures of the operations, and the third the measures of the corresponding results. If we wish to mark the primitive position of the radius OA as being in the positive direction, we should write $OA \times 1_\mu = +1 . 1_r . 1_\mu$; and as the measure of the result is all that is important to be recorded in this case, and may be understood to imply the measure of the operation by which that result is produced, the use of one character or sign is sufficient, and we shall have

$$\begin{aligned}
1_0 . 1_r . 1_\mu &= +1 . 1_r . 1_\mu, \\
1_0^2 . 1_r . 1_\mu &= (+1)^2 1_r . 1_\mu = +1 . 1_r . 1_\mu, \\
1_0^3 . 1_r . 1_\mu &= (+1)^3 1_r . 1_\mu = +1 . 1_r . 1_\mu, \\
1_0^4 . 1_r . 1_\mu &= (+1)^4 1_r . 1_\mu = +1 . 1_r . 1_\mu, \text{ etc.}
\end{aligned}$$

The successive results are identical in value, geometrical, dynamical, and arithmetical; that is, we have always $(+1)^n = +1$, or the involution of absolute geometrical unity does but repeat unity. Like that of successive multiplications by linear unity, the present process consists in making the material unit course uniformly through successive unts of space; but, unlike the former process, the units of space are superposed instead of juxtaposed, and thus give $1^n = 1$ in place of $1^n = n . 1$.

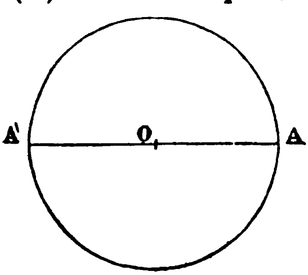
(12). The inverse operation which will correspond to division by absolute geometrical unity, and to the evolution of such unity when the operation is repeated, will be performed by revolving the radius

$1_r . 1_\mu = OA \times 1_\mu$ in the negative angular direction about the centre O, from the position OA to OA again. If the initial position $OA \times 1_\mu$ be denoted by $+1 . 1_r . 1_\mu$, the succession of results will be :

$$
\begin{aligned}
OA \times 1_\mu &= +1 . 1_r . 1_\mu, \\
\frac{OA}{AA} \times 1_\mu &= 1_0^{-1} . 1_r . 1_\mu = (+1)^{\frac{1}{2}} 1_r . 1_\mu = +1 . 1_r . 1_\mu, \\
\frac{OA}{AA} \times 1_\mu &= 1_0^{-2} . 1_r . 1_\mu = (+1)^{\frac{1}{3}} 1_r . 1_\mu = +1 . 1_r . 1_\mu, \\
\frac{OA}{AA} \times 1_\mu &= 1_0^{-3} . 1_r . 1_\mu = (+1)^{\frac{1}{4}} 1_r . 1_\mu = +1 . 1_r . 1_\mu, \\
\frac{OA}{AA} \times 1_\mu &= 1_0^{-4} . 1_r . 1_\mu = (+1)^{\frac{1}{5}} 1_r . 1_\mu = +1 . 1_r . 1_\mu, \text{ etc.}
\end{aligned}
$$

The second column expresses the measures of the successive operations in the arc of revolution ; for which is substituted, in the third column, the measures of the results in terms of the position of the radius, finally referred to their arithmetical values in the fourth column ; and thus positive unity has always one root equal to itself, or the equality $(+1)^{\frac{1}{n}} = +1$ is explained.

(13). In the second place, let the angular multiplying unit be that which is measured by the semi-circumference. The initial position being $OA \times 1_\mu$ as before, one step of the compasses will place us in the position $OA' \times 1_\mu$, and the step repeated will restore $OA \times 1_\mu$; the revolution, as usual, being had in the positive angular direction. Denoting the multiplying unit arc AA′ by 1_π, the successive results will be :



$$
\begin{aligned}
OA \times 1_\mu &= 1_r . 1_\mu, \\
AA' \times OA \times 1_\mu &= OA' \times 1_\mu = 1_\pi . 1_r . 1_\mu, \\
A'A \times OA' \times 1_\mu &= OA \times 1_\mu = 1_\pi^2 . 1_r . 1_\mu, \\
AA' \times OA \times 1_\mu &= OA' \times 1_\mu = 1_\pi^3 . 1_r . 1_\mu, \\
A'A \times OA' \times 1_\mu &= OA \times 1_\mu = 1_\pi^4 . 1_r . 1_\mu, \text{ etc.}
\end{aligned}
$$

Now when contrasting the operation of addition with its inverse operation, subtraction, we have found their unit measures to be

geometrically represented (constructed) by these very lines OA and OA′ respectively, drawn in opposite directions from the same origin O; that is to say, starting from zero at O, we have found that in multiplying by a positive linear unit we get $+1.1_\lambda.1_\mu = \mathrm{OA}\times 1_\mu$, and similarly $-1.1_\lambda.1_\mu = \mathrm{OA}'\times 1_\mu$ in multiplying by a negative linear unit. Then denoting the measure of the operation by the same character which denotes the measure of the result, we first convert the preceding columns thus:

$$
\begin{aligned}
&& \mathrm{OA}\times 1_\mu &= +1.1_r.1_\mu,\\
1_\pi.1_r.1_\mu &= & \mathrm{OA}'\times 1_\mu &= -1.1_r.1_\mu,\\
1_\pi^2.1_r.1_\mu &= & \mathrm{OA}\times 1_\mu &= +1.1_r.1_\mu,\\
1_\pi^3.1_r.1_\mu &= & \mathrm{OA}'\times 1_\mu &= -1.1_r.1_\mu,\\
1_\pi^4.1_r.1_\mu &= & \mathrm{OA}\times 1_\mu &= +1.1_r.1_\mu, \text{ etc.}
\end{aligned}
$$

A cause is known only by its effect: we take the measure of the effect for the measure of the cause, and finally substitute this measure for the cause itself. The primitive position $\mathrm{OA}\times 1_\mu$ is expressed by $+1.1_r.1_\mu$; and as multiplication by 1_λ (the arc AA′ being the measure of the cause) yields the position expressed by $-1.1_r.1_\mu$ (the radius OA′ being the measure of the effect), we substitute the measure of the effect for the measure of the cause; and as in the first case of angular multiplication, when $+1$ was substituted for the measure of the operation 1_0, it stood for the circumference, while as measure of the result it represented the position of the radius in OA, so now we may substitute -1 for the present operation 1_π, in which case it will stand for the semicircumference, while as measure of the result it represents the position of the radius in OA′. We may then finally convert our columns thus:

$$
\begin{aligned}
&\mathrm{OA}\times 1_\mu = +1.1_r.1_\mu,\\
1_\pi(+1)1_r.1_\mu &= (-1)(+1)1_r.1_\mu = \mathrm{OA}'\times 1_\mu = -1.1_r.1_\mu,\\
1_\pi(-1)1_r.1_\mu &= (-1)(-1)1_r.1_\mu = \mathrm{OA}\times 1_\mu = +1.1_r.1_\mu = (-1)^2 1_r.1_\mu,\\
1_\pi(+1)1_r.1_\mu &= (-1)(+1)1_r.1_\mu = \mathrm{OA}'\times 1_\mu = -1.1_r.1_\mu = (-1)^3 1_r.1_\mu,\\
1_\pi(-1)1_r.1_\mu &= (-1)(-1)1_r.1_\mu = \mathrm{OA}\times 1_\mu = +1.1_r.1_\mu = (-1)^4 1_r.1_\mu,\\
&\text{etc.}
\end{aligned}
$$

Thus when negative unity is involved, we get $(-1)^n = +1$ and $(-1)^{n+1} = -1$, n being an even number and including 0.

(14). The inverse to the preceding operation will be performed by revolving by semicircumferential steps in the negative angular direction about the centre O, which gives the results:

$$+1.1_r.1_\mu = OA \times 1_\mu,$$

$$1_\pi^{-1}(+1)1_r.1_\mu = \frac{+1}{-1}.1_r.1_\mu = (+1)^{\frac{1}{2}}.1_r.1_\mu = -1.1_r.1_\mu = OA' \times 1_\mu,$$

$$1_\pi^{-2}(+1)1_r.1_\mu = \frac{-1}{-1}.1_r.1_\mu = (+1)^{\frac{1}{3}}.1_r.1_\mu = +1.1_r.1_\mu = OA \times 1_\mu,$$

$$1_\pi^{-3}(+1)1_r.1_\mu = \frac{+1}{-1}.1_r.1_\mu = (+1)^{\frac{1}{4}}.1_r.1_\mu = -1.1_r.1_\mu = OA' \times 1_\mu,$$

$$1_\pi^{-4}(+1)1_r.1_\mu = \frac{-1}{-1}.1_r.1_\mu = (+1)^{\frac{1}{5}}.1_r.1_\mu = +1.1_r.1_\mu = OA \times 1_\mu, \text{ \&c.}$$

Thus positive unity has always negative unity for one of its roots of the nth degree, when n is an even number, not otherwise.

(15). By comparing conclusions, we find in the preceding examples of multiplication by the circumference and by the semicircumference, the demonstration of the proposition that the product of like signs is positive, and that of unlike signs is negative; that is, $(+1)(+1) = +1$ and $(-1)(-1) = +1$, but $(+1)(-1) = -1$ and $(-1)(+1) = -1$; and further, that $(+1)\div(+1) = +1$ and $(-1)\div(-1) = +1$, but $(+1)\div(-1) = -1$ and $(-1)\div(+1) = -1$.

(15). As a general illustration of multiplication by positive and negative unity, let me be required, first, to multiply the number N by $+1$. I am asked to perform an operation, and it is meet that I should record the measure of that operation. In order that N, which is an abstract number, may afford something tangible that may be operated upon, it must be made the coefficient of some material unit 1_μ; and the operation might at first be performed by carrying $N1_\mu$ through the distance $OP = 1_l$ the linear unit, in the positive direction; since the measure of the operation would be $+1N1_l$, which expresses an operation that is measured by a unit of space. But as the linear unit is an indeterminate magnitude, a more general, and indeed an absolute unit of operation will be performed by revolving $N1_l.1_\mu$ through the circumference, the absolute unit of space; in which case the expression $+1N1_\mu$ implies the performance of one revolution, which fulfils the condition of an absolute unit of operation upon $N1_\mu$; and, accordingly as the sign $+1$ governs in the active or the passive sense (distinguished as $+1.1_\lambda$ and $+1.1_l$), it will command such revolution, or express its result.

O ———— P

If required to multiply N by -1, I am asked to perform an

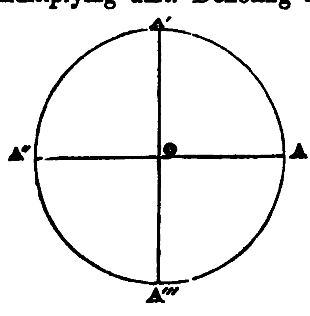
operation which shall give a negative result; that is, a result that would eliminate a positive result equal to it in magnitude, by mutual destruction. Equal and directly opposing forces destroy each other's effects, but the measures of the separate effect of each force are truly exhibited by two equal lines drawn in diametrical opposition from the same centre. If, therefore, the measure of the operation expressed

by $+1.1_{\prime}.N1_{\mu}$ is truly represented by the direct line OP, and I must convert this expression into $-1.1_{\prime}.N1_{\mu}$ (such as would destroy the effect $+1.1_{\prime}.N1_{\mu}$), it will be accomplished by a unit of operation, by one single factor which does not change the value of its measure during its action, by one semirevolution about the centre O; and now accordingly as -1 has its active or passive sense (distinguished as -1.1_{λ} and $-1.1_{\prime}$), it will command such semirevolution, or express its result.

Each of the questions demands the performance of a unit of operation upon the concrete quantity $N1_{\mu}$, its multiplication by a factor unity; and each of these demands, together with that of the mutual opposition of the quality of the results, is fully satisfied, and, it is hoped, in a rational manner.

The common rule for subtraction in algebra directs the sign of each term of the subtrahend to be changed, and the result to be added to the minuend. What is the first clause of this rule, other than a direction to revolve each term of the subtrahend through an arc of 180°, so as to place the whole in mechanical opposition to the minuend?

(17). The third and last form of circular multiplication to be discussed, is that in which the right angle, expressed by the quadrant as its measure, is adopted as the multiplying unit. Denoting the multiplier, the arc AA′ of 90°, by 1_{a}; since the direct operations are perfectly understood from what has preceded, it is only necessary to write the steps and results as follows (beginning, as in all the other cases, with



$$OA\times 1_{\mu} = +1.1_{r}.1_{\mu}):$$

$$\begin{aligned}
+1.1_r.1_\mu &= OA \times 1_\mu, \\
1_\alpha(+1)1_r.1_\mu &= OA' \times 1_\mu, \\
1_\alpha^2(+1)1_r.1_\mu &= OA'' \times 1_\mu = -1.1_r.1_\mu, \\
1_\alpha^3(+1)1_r.1_\mu &= OA''' \times 1_\mu, \\
1_\alpha^4(+1)1_r.1_\mu &= OA \times 1_\mu = +1.1_r.1_\mu, \\
1_\alpha^5(+1)1_r.1_\mu &= OA' \times 1_\mu = 1_\alpha(+1)1_r.1_\mu, \text{ etc.}
\end{aligned}$$

The first multiplication places the radius in the perpendicular position OA′, in which, while it has the value unity in its present direction, its value is neutral (between positive and negative) or zero in its primitive direction; that is, the result of the operation has at once the value zero in the primitive direction (or on the axis through A″A), and the value unity in the perpendicular direction (or on the axis through A‴A′)*. A second multiplication gives the position OA″, where the result has the value negative unity, or -1.1_r on the primitive axis A″A. A third multiplication yields the position OA‴, again neutral or zero on the primitive axis, but negative with respect to the first position OA′ on the perpendicular axis A‴A′. Finally the fourth multiplication finds the primitive position OA, expressed by $+1.1_r$; and a further continuance would but repeat former results.

Now it has abundantly appeared in this our method of successive multiplication by unity, that the square of a geometrical unit (whether linear or angular) is a (similar) geometrical unit *twice* the magnitude of the root (or factor squared), the cube is *thrice* the magnitude of the root, the biquadrate is *four times* its root, and so on. Inversely, therefore, the square root of a geometrical unit is a similar unit of *half* the magnitude of the unit submitted to evolution, the cube root is *one-third* the magnitude of the original unit, the biquadrate root is *one-fourth* its original, and so on. So that if the primitive geometrical unit consists of four right angles, its square root will consist of two right angles, and its biquadrate root of one right angle; or if the primitive unit consists of two right angles, its square root will be one right angle†.

(18). Having found by direct process, when the right angle

* This geometrical fact constitutes the principle of perpendicularity, and is of capital importance in the calculus of operations.

† The *apparent* exception offered by the involution and evolution of the absolute geometrical unit, is occasioned by the principle of superposition.

(denoted by 1_α) is the geometrical multiplier, the result

$$1^2_\alpha . 1_r . 1_\mu = OA'' \times 1_\mu = -1 . 1_r . 1_\mu,$$

where the last term expresses the measure of the result and implies the measure of the operation, we may now apply the inverse process to this result, and obtain

$$OA' \times 1_\mu = (1^2_\alpha)^{\frac{1}{2}} 1_r . 1_\mu = 1_\alpha . 1_r . 1_\mu = (-1)^{\frac{1}{2}} 1_r . 1_\mu = +\sqrt{(-1)} 1_r . 1_\mu .$$

By using the notation $+\sqrt{(-1)}1_r$ for OA′, and understanding that the measure of the effect here also at the same time implies the measure of the operation or cause which produces that effect, we convert the last preceding columns as follows :

$$+1 . 1_r . 1_\mu = OA \times 1_\mu,$$

$$(+\sqrt{-1})\ (+1)1_r . 1_\mu = +\sqrt{(-1)} 1_r . 1_\mu = OA' \times 1_\mu,$$

$$(+\sqrt{-1})^2 (+1) 1_r . 1_\mu = (-1)(+1) 1_r . 1_\mu = -1 . 1_r . 1_\mu = OA'' \times 1_\mu,$$

$$(+\sqrt{-1})^3 (+1) 1_r . 1_\mu = (+\sqrt{-1})(-1) 1_r . 1_\mu = -\sqrt{(-1)} 1_r . 1_\mu = OA''' \times 1_\mu,$$

$$(+\sqrt{-1})^4 (+1) 1_r . 1_\mu = (+\sqrt{-1})(-\sqrt{-1}) 1_r . 1_\mu = +1 . 1_r . 1_\mu = OA \times 1_\mu,$$

$$(+\sqrt{-1})^5 1_r . 1_\mu = +\sqrt{(-1)} 1_r . 1_\mu = OA' \times 1_\mu, \text{ etc.}$$

By proceeding from the primitive position OA in the negative angular direction, we make the results :

$$OA \times 1_\mu = +1 . 1_r . 1_\mu,$$

$$\frac{+1}{+\sqrt{(-1)}} . 1_r . 1_\mu = OA''' \times 1_\mu = -\sqrt{(-1)} 1_r . 1_\mu,$$

$$\frac{-\sqrt{(-1)}}{+\sqrt{(-1)}} . 1_r . 1_\mu = OA'' \times 1_\mu = -1 . 1_r . 1_\mu,$$

$$\frac{-1}{+\sqrt{(-1)}} . 1_r . 1_\mu = OA' \times 1_\mu = +\sqrt{(-1)} 1_r . 1_\mu,$$

$$\frac{+\sqrt{(-1)}}{+\sqrt{(-1)}} . 1_r . 1_\mu = OA \times 1_\mu = +1 . 1_r . 1_\mu, \text{ etc.}$$

(19). The expression $+\sqrt{(-1)}$, which we have found to be geometrically represented by the quadrant when it is a multiplier or a *cause* (or when understood in *the active* sense), and by the perpen-

dicular radius unity when it is a product or *an effect* (or when understood in *the passive* sense), is the well-known primitive biquadrate root of positive unity, or as well the primitive square root of negative unity, which arises in analysis whenever the two factors of a negative product are sought.

The second biquadrate root of positive unity is $-$, and by direct operations it yields the following results :

$$+1.1_r.1_\mu = OA\times 1_\mu,$$
$$(-1)(+1)1_r.1_\mu = -1.1_r.1_\mu = OA''\times 1_\mu,$$
$$(-1)(-1)1_r.1_\mu = (-1)^2 1_r.1_\mu = +1.1_r.1_\mu = OA\times 1_\mu,$$
$$(-1)(+1)1_r.1_\mu = (-1)^3 1_r.1_\mu = -1.1_r.1_\mu = OA''\times 1_\mu,$$
$$(-1)(-1)1_r.1_\mu = (-1)^4 1_r.1_\mu = (+1)^2 1_r.1_\mu = +1.1_r.1_\mu = OA.1_\mu, \text{ \&c.}$$

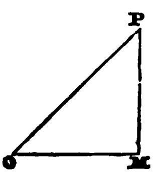
So that -1 is really the primitive biquadrate root of $(-1)^2$ the square of positive unity.

The third biquadrate root of positive unity is $-\sqrt{(-1)}$, and is represented geometrically by three right angles or the arc $\frac{3}{2}\pi$. By direct operations, its results follow thus :

$$OA\times 1_\mu = +1.1_r.1_\mu,$$
$$(-\sqrt{-1})(+1)1_r.1_\mu = OA'''\times 1_\mu = -\sqrt{(-1)}1_r.1_\mu,$$
$$(-\sqrt{-1})(-\sqrt{-1})1_r.1_\mu = OA''\times 1_\mu = -1.1_r.1_\mu,$$
$$(-\sqrt{-1})(-1)1_r.1_\mu = OA'\times 1_\mu = +\sqrt{(-1)}1_r.1_\mu,$$
$$(-\sqrt{-1})(+\sqrt{-1})1_r.1_\mu = OA\times 1_\mu = +1.1_r.1_\mu, \text{ etc.}$$

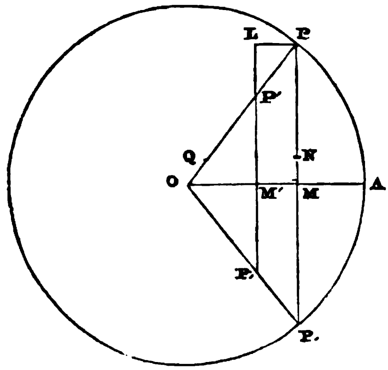
In this process it will be observed that the circumference has been thrice coursed, so that $-\sqrt{(-1)}$ is really the primitive biquadrate root of $(+1)^3$ the cube of positive unity.

(20). In the rightangled triangle OMP, let POM $= \theta$: then we



know the ratios $\frac{OM}{OP} = \cos\theta$ and $\frac{PM}{OP} = \sin\theta$; whence, conversely, the numerical values of the lines OM & PM are respectively $OP\times\cos\theta$ and $OP\times\sin\theta$; that is, where a line is multiplied by the cosine or by the sine of a given angle, its value is reduced to that of its projection through that angle in the first case, and through its complement in the second case. Then if OP be unity, that is, $OP = 1_{,}$, we have $OM = \cos\theta.1_{,}$

and PM $=\sin\theta.1_r$ in numerical value, or *quantity;* and these two last mentioned lines will have their algebraical notation, and be referred to the calculus of operations, by prefixing the appropriate signs of direction belonging to the positions in which they now stand. In the circle on the centre O, let the radius OA $= 1_r$, the linear unit, and the angle POA $= \theta$: then



$$\text{OM} = +1.\cos\theta.1_r,$$

$$\text{PM} = +\sqrt{(-1)}\sin\theta.1_r$$

and

$$\text{P}_{,}\text{M} = -\sqrt{(-1)}\sin\theta.1_r$$

in *quantity* and *quality.* The multiplicand 1_μ being at O, the multiplier $+1.\cos\theta.1_r$ brings it to the point M, whence it is carried either to P by the multiplier $+\sqrt{(-1)}\sin\theta.1_r$, or to P, by the multiplier $-\sqrt{(-1)}\sin\theta.1_r$. In the first case, the operation expressed by $(+1.\cos\theta+\sqrt{-1}.\sin\theta)1_r.1_\mu$ is equivalent to $\text{OP}\times 1_\mu$, or again to $\text{AP}\times 1_r.1_\mu$; and in the latter case, the operation expressed by $(+1.\cos\theta-\sqrt{-1}.\sin\theta)1_r.1_\mu$ is equivalent to $\text{OP}'\times 1_\mu$, or again to $\text{APP}_{,}\times 1_r.1_\mu$ or $\text{AP}_{,}\times 1_r.1_\mu$. Then we should have

$$(+1.\cos\theta+\sqrt{-1}.\sin\theta)1_r \times (+1.\cos\theta-\sqrt{-1}.\sin\theta)1_r.1_\mu$$

$$\text{equal to } \text{AP}\times\text{PP}_{,}\text{A}\times 1_r.1_\mu = \text{OA}\times 1_\mu, \text{ and}$$

$$\frac{(+1.\cos\theta+\sqrt{-1}.\sin\theta)1_r}{(+1.\cos\theta+\sqrt{-1}.\sin\theta)1_r}.1_\mu$$

$$\text{equal to } \text{AP}\times\text{PA}\times 1_r.1_\mu = \text{AP}\times\text{AP}_{,}\times 1_r.1_\mu = \text{OA}\times 1_\mu.$$

Take OP′ $=$ OM, and PQ $=$ PM : the first, separately projected through the angle θ and its complement, gives OM′ $= \cos^2\theta.1_r$ and P′M′ $= \cos\theta.\sin\theta.1_r$; and the second, projected through the complement of θ, gives PN $= \sin^2\theta.1_r$.

Effecting the actual multiplication of the two factors

$$(+1.\cos\theta+\sqrt{-1}.\sin\theta) \text{ and } (+1.\cos\theta-\sqrt{-1}.\sin\theta),$$

we get the terms

$$+1.\cos\theta\times+1.\cos\theta,$$

$$+\sqrt{-1}.\cos\theta.\sin\theta, \qquad -\sqrt{-1}.\cos\theta.\sin\theta,$$

$$+\sqrt{-1}.\sin\theta\times-\sqrt{-1}.\sin\theta.$$

1° $+1.\cos\theta.1_r$ carries 1_μ from O to M, and $+1.\cos\theta.1_r$ revolves the result through four right angles, and reduces it to........ $OM' = +1.\cos^2\theta.1_r.1_\mu$.

2° $+\sqrt{-1}.\cos\theta.\sin\theta.1_r$ would carry 1_μ from M′ to P′, but at the same time $-\sqrt{-1}.\cos\theta.\sin\theta.1_r$ would carry it to $P_{\prime}'$: consequently in virtue of the two equal opposing factors, 1_μ will remain at M′.

3° $+\sqrt{-1}.\sin\theta.1_r$ carries 1_μ from M′ to L (M′L = MP), and $-\sqrt{-1}.\sin\theta.1_r$ revolves the result through three right angles, and reduces it to $M'A = PN = +1.\sin^2\theta.1_r.1_\mu$.

Then $OM'+M'A = (+1.\cos^2\theta+1.\sin^2\theta)1_r = +1.1_r$. Thus is effected the complete synthesis of the formula $\cos^2\theta + \sin^2\theta = 1$, which is the numerical analogue of the forty-seventh proposition of the first book of Euclid.

(21). The functions of unity,

$$+1.\cos\theta+\sqrt{-1}.\sin\theta \quad \text{and} \quad +1.\cos\theta-\sqrt{-1}.\sin\theta,$$

whose synthesis and geometrical construction are above given, are coefficients referring the radius 1_r respectively to the positious OP and $OP_{\prime}$, forming the angles $+\theta$ and $-\theta$ with the primitive position OA : they are obtained by multiplication, and expressed in terms of the primitive and perpendicular axes and the angle θ; their product is absolute unity, geometrical, mechanical, and arithmetical; and the separate result or effect expressed by each is identical with that which would be produced by the multiplication of $1_r.1_\mu$ by the circular factor $+\theta$ or $-\theta$. If then we can find two factors respectively equal to $+\theta$ and $-\theta$, and whose product shall at the same time be equal to 1, they will be equivalent to the factors

$$+1.\cos\theta+\sqrt{-1}.\sin\theta \quad \text{and} \quad +1.\cos\theta-\sqrt{-1}.\sin\theta,$$

and may be expressed in terms of the angle θ alone. Now the exponential functions $e^{+\sqrt{-1}.\theta}$ and $e^{-\sqrt{-1}.\theta}$ (e being the base of napierian logarithms) are two factors whose product $e^{+\sqrt{-1}.\theta} \times e^{-\sqrt{-1}.\theta} = e^0$ is numerical unity, and we are to inquire if they can be shown to be equivalent respectively to $+\theta$ and $-\theta$ as coefficients of the radius 1_r in the positions OP and $OP_{\prime}$.

The most general interpretation that can be given of the signs $\pm\sqrt{(-1)}$, is that they denote unit measures of quantities that are null on the primitive axis of measurement. Now angles and lines are heterogeneous elements; and although they can be so placed that one may be accepted as a true measure of the other, yet they are really mutually independent, and one forms no part or constituent at

all of the other : that is, one is zero with respect to the other. When the sign $\sqrt{(-1)}$ is a coefficient of a straight line, it indicates the perpendicular position of that line as the result of an operation, and consequently is the coefficient of a quantity which has its own value in its own sense, but is zero with respect to the primitive axis of measurement; but since angles and lines are zero with respect to each other, and yet are mutually measurable by each other, the sign $\sqrt{(-1)}$, when made the coefficient of an angle (or of the arc which is its measure), at first does nothing more than put in evidence the native heterogeneity of the elements angle and line, namely, that the angle is nothing among lines, and *vice versa* : it is the coefficient of a primitive quantity which has its own value in its own sense, but is zero with respect to the linear axis of measurement.

(22). When any number, as e, is to be involved geometrically, it must be made the coefficient of a unit of space; and, by the calculus of operations, the exponent of the number becomes the coefficient of the unit of space 1_λ, the suffix $_\lambda$ denoting that the unit of space is a multiplier and not a multiplicand.

1° The general result is $e^n . 1_\lambda^n = e^n . n1_l$ or $e^n . n1_v$, accordingly as the unit of space is rectilinear or circular. The result $e^n . n1_l$ is equal to the line $n1_l$ when e is unity, and has a determinate relation to that quantity when e is different from unity, that is, a relation which can be found by actual development.

2° If the exponent be $\pm\sqrt{(-1)}n$, the involution of the number e will give $e^{\pm\sqrt{(-1)}n} \times \pm\sqrt{(-1)}n . 1_l$; which, by reason that $\pm\sqrt{(-1)}$ is zero on the primitive axis, will be $e^0 \times 0 . 1_l$ equal to zero on that axis; but since $\pm\sqrt{(-1)} = \pm 1$ on the perpendicular axis, the concrete function will have its full value on that axis. Consequently $e^{\pm\sqrt{(-1)}n}$ has no direct development upon the primitive axis, but may have an indirect one through the perpendicular.

3° If we take the radius $1_r = 1_l$ the linear unit, and substitute an angle (by its arc $\theta 1_r$) for the exponent n, this exponent θ will be a number expressing how many times the given arc $\theta 1_r$ contains the radius; and the involution $e^\theta . 1_\lambda^\theta = e^\theta . \theta 1_r$ will have a determinate relation to $\theta 1_r$, to be found by development, and measurable directly upon the primitive axis.

4° By substituting $\pm\sqrt{(-1)}\theta$ for exponent, nothing will be changed in the relation of the angle or arc to the radius, the direction merely from the primitive axis being indicated by the double sign; and the involution becomes $e^{\pm\sqrt{(-1)}\theta} \times \pm\sqrt{(-1)}\theta 1_r$, which has

also a determinate relation to $\pm 1.\theta 1_r$, to be found by development, but measurable directly upon the perpendicular axis.

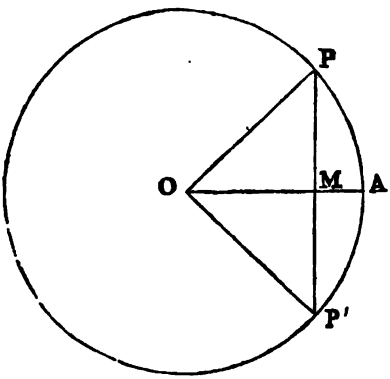
5° But $e^{\pm\sqrt{(-1)}\theta}$ may also be interpreted as $(e^{\pm\sqrt{(-1)}})^\theta$; and since we may stipulate that the exponent shall always express *number of times*, the sign must be held to its *real* or numerical value zero in the exponent (because $\frac{(\pm\sqrt{-1})1_l}{1_l} = 0$, just as $\frac{\theta 1_r}{1_r} = \theta$), while its *imaginary* or algebraical value perpendicular unity remains in the coefficient of the multiplying unit of space. Then $e^{\pm\sqrt{(-1)}} = e^0 = 1$, and the involution will be $e^{\pm\sqrt{(-1)}\theta}\times(\pm\sqrt{-1})\theta 1_r = (e^0)^\theta\times(\pm\sqrt{-1})\theta 1_r$

$= 1^\theta\times(\pm\sqrt{-1})\theta 1_r$

$= 1\times(\pm\sqrt{-1})\theta 1_r$

$= \pm 1.\theta 1_r$

$=$ OP and OP$_{\prime\prime}$, since the arc or angle is not at all disturbed by the sign of imaginarity, and its direction from the primitive axis is indicated by the positive or negative sign.



I say then that just as $1^n.1^n_\lambda = 1.n1_l$ in linear involution, so does $e^{+\sqrt{(-1)}\theta}\times 1_r{}^{+\sqrt{(-1)}\theta} = 1\times(+\sqrt{-1})\theta 1_r = +1.\theta 1_r$ in circular involution, the arc equal to the radius unity being the unit of arc. Therefore the arc AP, which measures the angle POA, both of which are commonly expressed by θ, is the measure of the operation which generates the exponential function $e^{+\sqrt{(-1)}\theta}$; and consequently when the loaded radius OA$\times 1_\mu = 1_r.1_\mu$ is the multiplicand, and the above function is the multiplier, the complete concrete function (the unit of arc being always equal to the radius unity)

$$e^{+\sqrt{(-1)}\theta}\times(+\sqrt{-1})\theta 1_r.1_\mu = +1.\theta 1_r.1_\mu = \text{AP}\times 1_r.1_\mu = \text{OP}\times 1_\mu,$$

and similarly

$$e^{-\sqrt{(-1)}\theta}\times(-\sqrt{-1})\theta 1_r.1_\mu = -1.\theta 1_r.1_\mu = \text{AP}'\times 1_r.1_\mu = \text{OP}'\times 1_\mu.$$

Thus we account for the agreement which has been found to subsist between the functions $e^{\pm\sqrt{(-1)}\theta}$ and $(+1.\cos\theta\pm\sqrt{-1}.\sin\theta)$; the former being expressed in terms of the angle alone, and the latter in terms of the angle and the axes. The product of each pair of factors [namely, $e^{+\sqrt{(-1)}\theta}\times e^{-\sqrt{(-1)}\theta} = 1$,

and $(+1.\cos\theta+\sqrt{-1}.\sin\theta)(+1.\cos\theta-\sqrt{-1}.\sin\theta) = 1$] is

equal to positive unity, and consequently returns $1_r . 1_\mu$ from either position $OP \times 1_\mu$ or $OP' \times 1_\mu$ to the primitive position

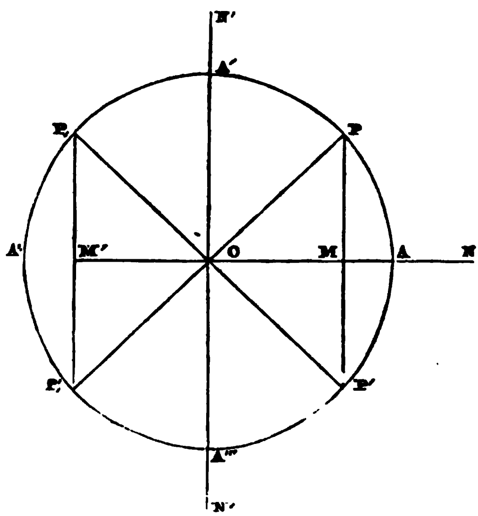
$$OA \times 1_\mu = +1 . 1_r . 1_\mu .$$

So while the factors $(+1 . \cos\theta \pm \sqrt{-1} . \sin\theta)1_r$ refer the material unit 1_μ from O, through OM and MP and MP′ respectively, to the points P and P′; the factors $e^{\pm\sqrt{(-1)}\theta} . 1_r$ refer 1_μ from A respectively through the arc AP or AP′, to the same points P and P′: the results are the same, but the operations are different.

(23). Since the double result $\pm 1 . \theta \, 1_r$ is obtained by involving the napierian base e to the power $\pm\sqrt{(-1)}\theta$, and is equal to the result $(+1 . \cos\theta \pm \sqrt{-1} . \sin\theta)1_r$, we see the explanation of the equation

$$\pm\sqrt{(-1)}\theta = \log(+1 . \cos\theta \pm \sqrt{-1} . \sin\theta).$$

As $e^{+\sqrt{(-1)}\theta} . 1_r = (+1 . \cos\theta + \sqrt{-1} . \sin\theta)1_r = OM + MP$, and $e^{-\sqrt{(-1)}\theta} . 1_r = (+1 . \cos\theta - \sqrt{-1} . \sin\theta)1_r = OM - MP'$, their sum $(e^{+\sqrt{(-1)}\theta} + e^{-\sqrt{(-1)}\theta})1_r = +2\cos\theta . 1_r = ON$, being the effect of



the factors in the direction of the primitive axis.

To subtract the second equation from the first, we must first refer OP′ through an angle of 180°, thus:

$-1 . e^{-\sqrt{(-1)}\theta} . 1_r = (-1 . \cos\theta + \sqrt{-1} . \sin\theta)1_r = OM' + M'P_, = OP_,$;

then adding to the first equation, we get

$(e^{+\sqrt{(-1)}\theta} - e^{-\sqrt{(-1)}\theta})1_r = +2\sqrt{(-1)}\sin\theta . 1_r = ON'$, being the effect of the factors in the direction of the perpendicular axis.

Similarly to subtract the first equation from the second, the first step is

$-1.e^{+\sqrt{(-1)}\theta}.1_r = (-1.\cos\theta - \sqrt{-1}.\sin\theta)1_r = \mathrm{OM}' - \mathrm{M}'\mathrm{P}_{\prime}{}' = \mathrm{OP}_{\prime}{}'$;

and then adding,

$$(-e^{+\sqrt{(-1)}\theta} + e^{-\sqrt{(-1)}\theta})1_r = -2\sqrt{(-1)}\sin\theta.1_r = \mathrm{ON}_{\prime}{}',$$

being a negative effect on the perpendicular axis.

The function $e^{\pm\sqrt{(-1)}\theta}$ is susceptible of two forms of involution, accordingly as we operate with $(e^{\pm\sqrt{(-1)}})^{\theta}$ or $(e^{\theta})^{\pm\sqrt{(-1)}}$: the first method gives the series $e^{\theta.\pm\sqrt{(-1)}}$, $e^{2\theta.\pm\sqrt{(-1)}}$, $e^{3\theta.\pm\sqrt{(-1)}}$, etc., referring to the angles $\pm\sqrt{(-1)}\theta 1_\theta$, $\pm\sqrt{(-1)}2\theta 1_\theta$, $\pm\sqrt{(-1)}3\theta 1_\theta$, etc. as measures of operation; while the second method gives $e^{\pm\sqrt{(-1)}\theta}$, $e^{-1.\theta}$, $e^{\mp\sqrt{(-1)}\theta}$, $e^{+1.\theta}$, etc., and refers to the lines $\pm\sqrt{(-1)}\theta 1_r$, $-1.\theta 1_r$, $\mp\sqrt{(-1)}\theta 1_r$, $+1.\theta 1_r$, etc. as measures of the results of operation. Then when $\theta = \frac{1}{2}\pi$, we have, by the first method,

$$\begin{aligned}
e^{\frac{1}{2}\pi.\pm\sqrt{(-1)}}.1_r &= \mathrm{OA}' \text{ or } \mathrm{OA}''' &&= \pm\sqrt{(-1)}1_r,\\
e^{\pi.\pm\sqrt{(-1)}}.1_r &= \mathrm{OA}'' &&= -1.1_r,\\
e^{(3.\frac{1}{2}\pi)\sqrt{-1}}.1_r &= \mathrm{OA}''' \text{ or } \mathrm{OA}' &&= \mp\sqrt{(-1)}1_r,\\
e^{2\pi.\pm\sqrt{(-1)}}.1_r &= \mathrm{OA} &&= +1.1_r;
\end{aligned}$$

whence

$$\begin{aligned}
\pm\tfrac{1}{2}\pi.\sqrt{(-1)}1_\theta &= \log(\pm\sqrt{-1}),\\
\pm 1\pi.\sqrt{(-1)}1_\theta &= \log(-1),\\
\pm\tfrac{3}{2}\pi.\sqrt{(-1)}1_\theta &= \log(\mp\sqrt{-1}),\\
\pm 2\pi.\sqrt{(-1)}1_\theta &= \log(+1),
\end{aligned}$$

1_θ being the arc equal to 1_r. And by the second method,

$$\begin{aligned}
e^{\pm\sqrt{(-1)}\frac{1}{2}\pi}.1_r &= \pm\sqrt{-1}.\tfrac{1}{2}\pi 1_{\prime} \quad (1_r \text{ being equal to } 1_{\prime}).\\
e^{-1.\frac{1}{2}\pi}.1_r &= -1.\tfrac{1}{2}\pi 1_{\prime\prime},\\
e^{\mp\sqrt{(-1)}\frac{1}{2}\pi}.1_r &= \mp\sqrt{-1}.\tfrac{1}{2}\pi 1_{\prime\prime},\\
e^{+1.\frac{1}{2}\pi}.1_r &= +1.\tfrac{1}{2}\pi 1_{\prime\prime},
\end{aligned}$$

the recorded results being merely linear. The two first logarithms (of $+\sqrt{-1}$ and -1) were first discovered by JOHN BERNOUILLI; and can now, with many other *imaginary* results equally curious, be interpreted geometrically.

(24). If in the equation $\cos^2\theta + \sin^2\theta = 1$ we write y and x for $\cos\theta$ and $\sin\theta$, and transpose the second term, we have

$$y^2 = 1 - x^2 = (1+x)(1-x).$$

Regarding y as the dependent and x as the independent variable, it is proposed to treat this equation by the calculus of operations, with a view to the further elucidation of the properties of the sign of

perpendicularity. To this end, both variables y and x must be referred to the same axis of measurement; but in order that successive values of y may be exhibited conveniently and distinctly, they may be traced parallel to the primitive axis, above that axis for positive and below for negative values of x, all the time understanding that the *true* place is also on the primitive axis.

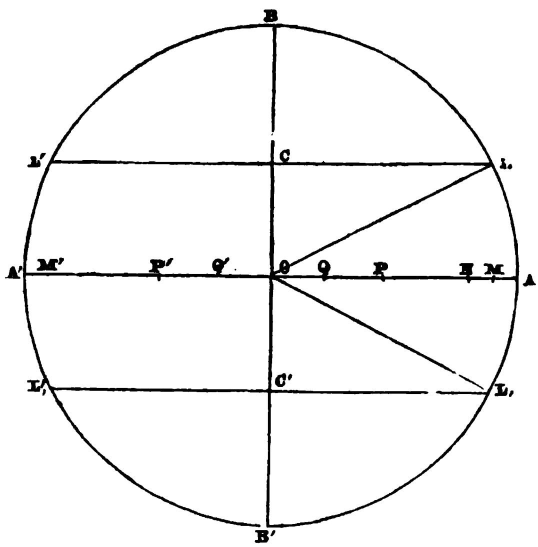
Let the linear unit $1_l = 1_\lambda$ be equal to OA; 1_λ being the linear, 1_0 the circumferential, and 1_π the semicircumferential unit multiplier, and 1_μ always the passive or material unit multiplicand. We have, then, OA $= +1.1_l$ in quantity and quality.

1° When $x = 0$, $y^2 1_l = (+1)^2 1_l = +1.1_l$, and

$$y 1_l = (+1)^{\frac{1}{2}} 1_l = \pm 1.1_l;$$

that is, $y 1_l =$ OA or OA′.

2° When x has increased to some positive value less than unity, let it be $x =$ OP, and take the first factor $1+x$ as coefficient to the multiplier 1_λ : the steps of the operation are,



$$+1.1_\lambda.1_\mu = \text{OA} \times 1_\mu = +1.1_l.1_\mu,$$
$$+1.x1_\lambda.1_\mu = \text{OP} \times 1_\mu = +1.x1_l.1_\mu.$$

Now this result $(+1+1x)1_l.1_\mu$ is our multiplicand, and the second factor $(1-x)$ is the coefficient of the new multiplier, which will be

circular as indicated by the signs $+1$ and -1 : the steps of the operation are,

$$\begin{array}{ll} +1.1_{\iota}.1_{\mu}+1.x1_{\iota}.1_{\mu} & = \text{multiplicand,} \\ 1_0 \text{ and } 1_{\pi}.x & = \text{multiplier.} \\ \hline +1.1_0.1_{\iota}.1_{\mu} & = +1.1_{\iota}.1_{\mu} = \text{OA}\times 1_{\mu}, \\ +1.1_0.x1_{\iota}.1_{\mu} & = +1.x1_{\iota}.1_{\mu} = \text{OP}\times 1_{\mu}, \\ +1.1_{\pi}.x1_{\iota}.1_{\mu} & = -1.x1_{\iota}.1_{\mu} = \text{OP}'\times 1_{\mu}, \\ +1.1_{\pi}.x^2 1_{\iota}.1_{\mu} & = -1.x^2 1_{\iota}.1_{\mu} = \text{OQ}'\times 1_{\mu}. \end{array}$$

The opposing equal factors OP and OP′ destroy each other's effects, and the result is $y^2 1_{\iota} = (+1-1x^2)1_{\iota} = +\text{OA}-\text{OQ} = +\text{ON}$.

Instead of the unequal factors $(+1+1x)1_{\lambda}=\text{OA}+\text{OP}=\text{A}'\text{P}$ and $(+1-1x)1_{\lambda}=\text{OA}-\text{OP}=\text{A}'\text{P}'$ which give the result $y^2 1_{\iota} = \text{ON}$, we might demand two equal ones, that is, the square root of

$$+\text{ON} = +1(1-x^2)1_{\iota};$$

and this will be found by extracting the square root both of the numerical factor $1-x^2$ (which expresses the measure of the result or quantity of the operation), and of the mechanical factor $+1$ (which measures the operation itself, or quality of the result). Then $y 1_{\iota} = (+1)^{\frac{1}{2}}(1-x^2)^{\frac{1}{2}}1_{\iota}$; and if OM be found equal to the numerical square root of ON, we have $y 1_{\iota} = (+1)^{\frac{1}{2}}.\text{OM}$. But we know that $(+1)^{\frac{1}{2}}$ has two values, to wit, $+1$ and -1; and therefore we have two different and equal quantities,

$$y 1_{\iota} = +1.\sqrt{(1-x^2)}1_{\iota} = \text{OM} \qquad \text{and}$$
$$y 1_{\iota} = -1.\sqrt{(1-x^2)}1_{\iota} = \text{OM}',$$

each of which squared will be equal to $+1(1-x^2)1_{\iota} = \text{ON}$.

The true place of both x and y is upon the primitive axis A′A; but if on the perpendicular axis B′B we take OC equal to any value OP of x whatever less than positive unity, and draw perpendicularly CL and CL′ each equal to OM the corresponding value of y, the extremities L and L′ will describe the quadrantal arcs AB and A′B, and OC and CL will measure the effect of the revolving unit radius OL upon the perpendicular and primary axes respectively.

3° If x decrease from zero towards negative unity, we have merely to invert the order of the factors into $(1-x)(1+x) = 1-x^2$. The first factor being $(+1-1x)1_{\lambda}$, the steps are :

$$+1.1_{\lambda}.1_{\mu} = \text{OA}\times 1_{\mu} = +1.1_{\iota}.1_{\mu},$$
$$-1.x1_{\lambda}.1_{\mu} = \text{OP}'\times 1_{\mu} = -1.x1_{\iota}.1_{\mu}.$$ The multipli-

cand being now $(+1-1x)1_{\prime}.1_{\mu}$, and $(+1+1x)$ the coefficient of the multiplier, the steps of the operation are :

$$(+1-1x)1_{\prime}.1_{\mu} \times (1_0 \text{ and } 1_0 x).$$

$$\begin{aligned}
+1.1_0.1_{\prime}.1_{\mu} &= +1.1_{\prime}.1_{\mu} &= \mathrm{OA}\times 1_{\mu},\\
-1.1_0.x1_{\prime}.1_{\mu} &= -1.x1_{\prime}.1_{\mu} &= \mathrm{OP'}\times 1_{\mu},\\
+1.1_0.x1_{\prime}.1_{\mu} &= +1.x1_{\prime}.1_{\mu} &= \mathrm{OP}\times 1_{\mu},\\
-1.1_0.x^2 1_{\prime}.1_{\mu} &= -1.x^2 1_{\prime}.1_{\mu} &= \mathrm{OQ'}\times 1_{\mu}.
\end{aligned}$$

The opposing equal factors OP′ and OP destroy each other, and the result is $y^2 1_{\prime} = (+1-1x^2)1_{\prime} = +\mathrm{OA}-\mathrm{OQ} = +\mathrm{ON}$ as before. But take OC′ equal to any value OP′ of $-x$ whatever less than negative unity, and draw perpendicularly $\mathrm{C'L}_{\prime}$ and $\mathrm{C'L}_{\prime}'$ each equal to OM the corresponding value of y, and the extremities $\mathrm{L}_{\prime}$ and $\mathrm{L}_{\prime}'$ will describe the quadrantal arcs AB′ and A′B′, while OC′ and $\mathrm{C'L}_{\prime}$ will measure the effect of the revolving unit radius $\mathrm{OL}_{\prime}$ upon the perpendicular and primary axes respectively.

4° Let x become equal to unity : then accordingly as x is equal to $+1$ or -1, the result of the first factor will be $(+1+1)1_{\prime}.1_{\mu}$ or $(+1-1)1_{\prime}.1_{\mu}$.

I. $(+1+1)1_{\prime}.1_{\mu} \times (1_0 \text{ and } 1_{\pi})$.

$$\begin{aligned}
+1.1_0.1_{\prime}.1_{\mu} &= +1.1_{\prime}.1_{\mu} &= \mathrm{OA}\times 1_{\mu},\\
+1.1_0.1_{\prime}.1_{\mu} &= +1.1_{\prime}.1_{\mu} &= \mathrm{OA}\times 1_{\mu},\\
+1.1_{\pi}.1_{\prime}.1_{\mu} &= -1.1_{\prime}.1_{\mu} &= \mathrm{OA'}\times 1_{\mu},\\
+1.1_{\pi}.1_{\prime}.1_{\mu} &= -1.1_{\prime}.1_{\mu} &= \mathrm{OA'}\times 1_{\mu}.
\end{aligned}$$

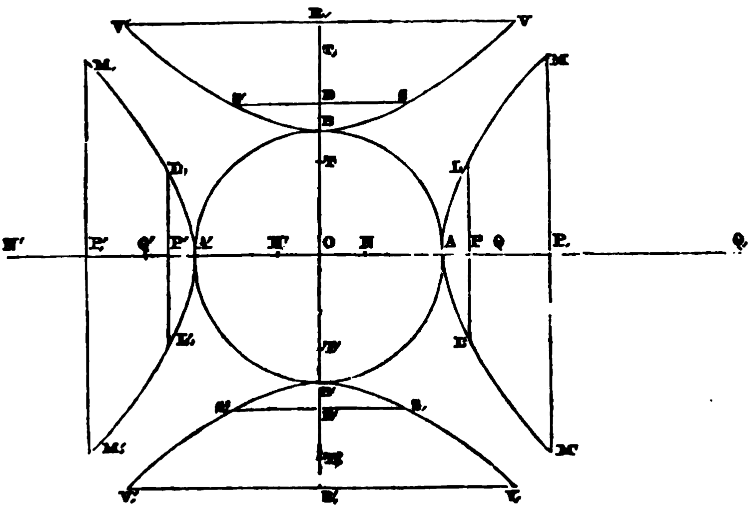
II. $(+1-1)1_{\prime}.1_{\mu} \times (1_0 \text{ and } 1_0)$.

$$\begin{aligned}
+1.1_0.1_{\prime}.1_{\mu} &= +1.1_{\prime}.1_{\mu} &= \mathrm{OA}\times 1_{\mu},\\
-1.1_0.1_{\prime}.1_{\mu} &= -1.1_{\prime}.1_{\mu} &= \mathrm{OA'}\times 1_{\mu},\\
+1.1_0.1_{\prime}.1_{\mu} &= +1.1_{\prime}.1_{\mu} &= \mathrm{OA}\times 1_{\mu},\\
-1.1_0.1_{\prime}.1_{\mu} &= -1.1_{\prime}.1_{\mu} &= \mathrm{OA'}\times 1_{\mu}.
\end{aligned}$$

The opposite steps destroy each other's effects, and $y^2 1_{\prime} = 0.1_{\prime}$. But in taking x perpendicular to the primitive axis as before, it becomes OB and OB′ respectively for the values $+1$ and -1, and in these positions measures the full unit value of the revolving radii OL and $\mathrm{OL}_{\prime}$. The points L and L′, and $\mathrm{L}_{\prime}$ and $\mathrm{L}_{\prime}'$, which marked the extent and position of the four values of y, have merged by pairs into the points B and B′, and consequently y is zero.

In all the preceding cases, x and y are the absciss and ordinate of the circle, or the sine and cosine of the variable arc $\mathrm{AL} = \theta$.

5° When x has become greater than positive unity, let it be equal to OP : the first factor $(+1+1x)1_{\lambda}$ gives



$$+1.1_{\lambda}.1_{\mu} = +1.1_{,}.1_{\mu} = \mathrm{OA}\times 1_{\mu},$$
$$+1.x1_{\lambda}.1_{\mu} = +1.x1_{,}.1_{\mu} = \mathrm{OP}\times 1_{\mu}.$$

And the second factor $(1_0=+1$ and $1_{\pi}=-1)$ gives

$$+1.1_0.1_{,}.1_{\mu} = +1.1_{,}.1_{\mu} = \mathrm{OA}\times 1_{\mu},$$
$$+1.1_0.x1_{,}.1_{\mu} = +1.x1_{,}.1_{\mu} = \mathrm{OP}\times 1_{\mu},$$
$$+1.1_{\pi}.x1_{,}.1_{\mu} = -1.x1_{,}.1_{\mu} = \mathrm{OP}'\times 1_{\mu},$$
$$+1.1_{\pi}.x^2 1_{,}.1_{\mu} = -1.x^2 1_{,}.1_{\mu} = \mathrm{OQ}'\times 1_{\mu}.$$

The result is $y^2 1_{,} = (+1-1x^2)1_{,} = +\mathrm{OA}-\mathrm{OQ}' = -\mathrm{ON}'$, a negative quantity; and, in fact, when $x > 1$, $1-x^2$ is negative. If a be the numerical value of $1-x^2$ independent of sign, then $1-x^2=-1a$, and $y1_{,} = (1-x^2)^{\frac{1}{2}}1_{,} = (-1)^{\frac{1}{2}}.\sqrt{a}.1_{,}$. Now we know that $(-1)^{\frac{1}{2}}$ has two values, to wit, $+\sqrt{(-1)}$ and $-\sqrt{(-1)}$. Therefore find the line equal to $\sqrt{a}.1_{,}$, and make

$$\mathrm{OT} = +\sqrt{(-1)}.\sqrt{(a)}1_{,}$$

and $\mathrm{OT}' = -\sqrt{(-1)}.\sqrt{(a)}1_{,}$, and they will be the two values of $y1_{,}$, each of which squared will be equal to

$$-1.a1_{,} = (1-x^2)1_{,} = \mathrm{ON}'.$$

Then if we take OP equal to any value whatever of x greater than

unity, and draw perpendicularly PL and PL′ equal to $(x^2-1)^{\frac{1}{2}}1_{\prime\prime}$, they will be respectively $(+\sqrt{-1})(x^2-1)^{\frac{1}{2}}1_{\prime}$ and $(-\sqrt{-1})(x^2-1)^{\frac{1}{2}}1_{\prime}$; and the points L and L′ will describe an equilateral hyperbola, whose vertex is at A where $x=1$ and $y=0$.

6° If x were negatively greater than -1, we should use the factors in the order $(1-x)$ and $(1+x)$, when we should construct the opposite equilateral hyperbola whose vertex is at A′, where $x=-1$ and $y=0$.

7° Now when $x>1$, the result $(1-x^2)1_{\prime}$ is negative, and equal to $(x^2-1)(-1)1_{\prime}$. For instance, when $x^2 1_{\prime} = \mathrm{OQ}$, we have found $(1-x^2)1_{\prime} = -\mathrm{ON}'$, which is evidently $(+\mathrm{ON})(-1)$. Then if we multiply $(1-x^2)1_{\prime}$ by -1, it becomes $+\mathrm{ON} = (x^2-1)1_{\prime\prime}$, equal to $(x+1)(x-1)1_{\prime}$; and now by constructing $y1_{\prime} = \pm 1(x^2-1)^{\frac{1}{2}}1_{\prime}$ parallel to the primitive axis, first for the factors in their present order, and afterwards transposed, we shall describe the two opposite equilateral hyperbolæ whose vertices are at B and B′, where $y=0$ and $x=\pm 1$, although the *true* place of both x and y is on the primitive axis A′A.

8° By operating with the factors $(x+1)1_{\lambda}$ and $(x-1)1_{\lambda}$ of the equation $(x^2-1)1_{\prime} = y^2 1_{\prime\prime}$, we move as follows [figure on next page]:

For values of x greater than $+1$ and less than -1, we shall find y to be the ordinates of the two hyperbolæ whose vertices are B and B′; but when x is less than $+1$ and greater than 0, let it be $x1_{\prime} = \mathrm{OP}$.

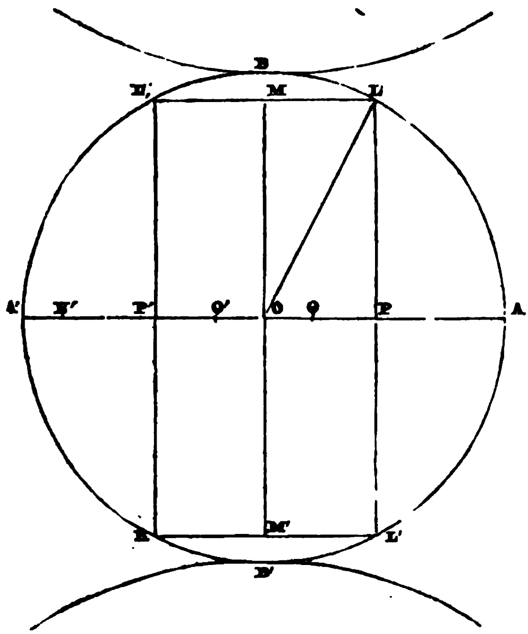
Then
$$+1.x1_{\lambda}.1_{\mu} = +1.x1_{\prime}.1_{\mu} = \mathrm{OP}\times 1_{\mu},$$
$$+1.1_{\lambda}.1_{\mu} = +1.1_{\prime}.1_{\mu} = \mathrm{OA}\times 1_{\mu}.$$

$$\begin{array}{ll}
+1.x1_{\prime}.1_{\mu} +1.1_{\prime}.1_{\mu} & = \text{multiplicand}, \\
\quad 1_0 x \text{ and } 1_{\pi} & = \text{multiplier}. \\
\hline
+1.1_0.x^2 1_{\prime}.1_{\mu} & = +1.x^2 1_{\prime}.1_{\mu} = \mathrm{OQ}\times 1_{\mu}, \\
+1.1_0.x1_{\prime}.1_{\mu} & = +1.x1_{\prime}.1_{\mu} = \mathrm{OP}\times 1_{\mu}, \\
+1.1_{\pi}.x1_{\prime}.1_{\mu} & = -1.x1_{\prime}.1_{\mu} = \mathrm{OP}'\times 1_{\mu}, \\
+1.1_{\pi}.1_{\prime}.1_{\mu} & = -1.1_{\prime}.1_{\mu} = \mathrm{OA}'\times 1_{\mu}.
\end{array}$$

The opposing equal factors OP and OP′ destroy each other's effects, and the result is

$$-\mathrm{OA}'+\mathrm{OQ} = -\mathrm{ON}' = (-1+1x^2)1_{\prime} = (1-x^2)(-1)1_{\prime} = y^2 1_{\prime}.$$

Then $y1_{\prime} = \pm\sqrt{(-1)}(1-x^2)^{\frac{1}{2}}1_{\prime} = \mathrm{OM}$ and OM′, or PL and PL′; and if the order of the factors were $(x-1)1_{\lambda}$ and $(x+1)1_{\lambda}$, we should have precisely the same result; but by taking x negative and equal



to OP′, we shall get $y l_{,} = P'L_{,}$ and $P'L_{,}'$. Then giving to x all values between ± 1 and 0, the points L and L′, $L_{,}$ and $L_{,}'$, will describe the two semicircles BAB′ and BA′B′, under the imaginary ordinate y. This ordinate y is the sine of the variable arc AL, x being its cosine; and thus is effected the transition from the equilateral hyperbola to the circle.

(25). We began with the equation $x^2+y^2=1$, or $\cos^2\theta+\sin^2\theta=1$, the sine and cosine being of course each less than unity; and when x or $\cos\theta$ becomes greater (either positively or negatively) than unity, since the square of a positive or negative quantity is alike positive, it is also greater than unity, and the square of y or $\sin\theta$ must necessarily be taken negatively in order to satisfy the equation, which now becomes $(\pm x)^2+(-y^2)=1$, or $(\pm\cos\theta)^2+(-\sin^2\theta)=1$, that is, $x^2-y^2=1$ or $\cos^2\theta-\sin^2\theta=1$; or the *difference* of the squares of the variables equal to unity, whereas before their *sum* equalled unity. In the equation $x^2+y^2=1$, the limits of x and y are 0 and ± 1 for both, and the sines and cosines appertain to the circle; but in the equatlon $x^2-y^2=1$, the limits of x are ± 1 and $\pm\infty$, those of y being 0 and $\pm\infty \mp 1$, and the sines and cosines appertain to the hyperbola.

Thus we see that the transition of x across unity necessarily converts the equation $x^2+y^2=1$ into $x^2-y^2=1$, in order that the condition of equality to unity may still be satisfied. Therefore there are two alternatives: either y^2 must be multiplied by -1 in the original equation $x^2+y^2=1$ or $y^2=1-x^2$, which will convert it into $x^2-y^2=1$ or $y^2=x^2-1$; or else y must be multiplied by $\surd(-1)$ in the decomposed equation $(x+\surd-1.y)(x-\surd-1.y)=1$ or $y=\surd(1-x^2)$, which will give the results
$(x+\surd-.1y.\surd-1)(x-\surd-1.y.\surd-1)=(x-y)(x+y)=x^2-y^2=1$, and
$(y.\surd-1)^2=1-x^2$, which is $y^2=-1(1-x^2)=+1(x^2-1)$.

(26). Elevating the equations $e^{+\surd-1.\theta}=+1.\cos\theta+\surd-1.\sin\theta$ and
$e^{-\surd-1.\theta}=+1.\cos\theta-\surd-1.\sin\theta$ to
the power $+\surd-1$, they become

$e^{-1.\theta}=+1.\cos(\surd-1.\theta)+\surd-1.\sin(\surd-1.\theta)$ and

$e^{+1.\theta}=+1.\cos(\surd-1.\theta)-\surd-1.\sin(\surd-1.\theta)$, by reason of the truth of Demoivre's formula

$$(\cos\theta\pm\surd-1\sin\theta)^n=\cos n\theta\pm\surd-1\sin n\theta$$

for all values of n. But the sign $\surd-1$ being nothing else but unity with respect to angle or arc, the righthand members of the two last equations are the same as $+1.\cos\theta(\surd-1)\pm\surd-1.\sin\theta(\surd-1)$; and this again is $+\surd-1.\cos\theta\mp1.\sin\theta$, from which we obtain the product $-1.\cos^2\theta-1.\sin^2\theta=-1$, that is, $+1.\cos^2\theta+1.\sin^2\theta=+1$; and we have also $e^{-1.\theta}\times e^{+1.\theta}=\pm1=(+1)^{\frac{1}{2}}$.

The double value $e^{-1.\theta}\times e^{+1.\theta}=\pm1$ arises thus:

1° The value $+1$ arises from the reciprocality of the factors, as in all cases $a^n\times a^{-n}=\frac{a^n}{a^n}=+1$.

2° The value -1 arises from geometrical involution, by which $e^{\pm\surd-1.\theta}.1_\lambda^{\pm\surd-1.\theta}=e^{\pm\surd-1.\theta}\times(\pm\surd-1)\theta1_\iota$; and as $e^{\pm\surd-1.\theta}=e^0=1$ (because $\surd-1=0$, since exponents are numerical values obtained from the primitive axis), this result is $(\pm\surd-1)\theta1_\iota$ on the perpendicular axis. Then we have $e^{+\surd-1.\theta}\times e^{-\surd-1.\theta}.1_\iota=+1.1_\iota$ by reciprocality, and (regarding $\theta1_\lambda$ as a multiplicand)

$$e^{+\surd-1.\theta}\times e^{-\surd-1.\theta}.\theta1_\lambda=(+\surd-1)(-\surd-1)\theta1_\iota=+1.1_\iota$$

also by the factors obtained from the involution.

3° But as we have now $e^{+\surd-1}=+\surd-1$ and $e^{-\surd-1}=-\surd-1$, if we square the first equation and bisquare the second, it will be equivalent to multiplying the exponents and coefficients by

$+\sqrt{-1} = (-1)^{\frac{1}{2}} = (+1)^{\frac{1}{4}}$, and gives

$(e^{+\sqrt{-1}})^2 = e^{-1} = (+\sqrt{-1})^2 = -1$, and

$(e^{-\sqrt{-1}})^4 = e^{+1} = (-\sqrt{-1})^4 = +1$. Consequently by reciprocality, $e^{-1.\theta} \times e^{+1.\theta}.1_{\prime} = +1.1_{\prime}$; but by the factors obtained from the involution (regarding $\theta 1_{\lambda}$ as a multiplicand), we get

$e^{-1.\theta} \times e^{+1.\theta}.\theta 1_{\lambda} = (-1)(+1)\theta 1_{\prime} = -1.\theta 1_{\prime}$; that is,

$e^{-1.\theta} \times e^{+1.\theta} = \pm 1$, accordingly as the factors are involved arithmetically or geometrically.

The same result is also deducible immediately from the comparison of the geometrical with the arithmetical involution of the factors of the lefthand member of the last equation, the first of which gives $e^{\mp 1.\theta}.1_{\lambda}^{\mp 1.\theta} = e^{\mp 1.\theta} \times \mp 1.\theta 1_{\prime}$; and now $\mp 1.\theta 1_{\prime}$ represents two lines upon the primitive axis, and may be made the multiplicand: whence

$$e^{-1.\theta} \times e^{+1.\theta}.\theta 1_{\prime} = e^{-1.\theta}.(-1) \times e^{+1.\theta}.(+1)\theta 1_{\prime} = 1 \times (-1)(+1)\theta 1_{\prime}$$
$$= -1.\theta 1_{\prime\prime}, \text{ or}$$

$e^{-1.\theta} \times e^{+1.\theta} = -1$, while the arithmetical involution gives

$e^{-1.\theta} \times e^{+1.\theta} = +1$*.

The double factor $+1.\cos\theta(\sqrt{-1}) \pm \sqrt{-1}.\sin\theta(\sqrt{-1})$, expressed in co-ordinates, is $x(\sqrt{-1}) \pm \sqrt{-1}.y(\sqrt{-1})$, which gives

$$[x(\sqrt{-1})+\sqrt{-1}.y(\sqrt{-1})].[x(\sqrt{-1})-\sqrt{-1}.y(\sqrt{-1})] = -x^2-y^2$$
$$= -1.$$

In this stage of the proceeding, if we multiply $+\sqrt{-1}.\cos\theta$, and $x(\sqrt{-1})$, by $-\sqrt{-1}$, we convert the double factors

$$+\sqrt{-1}.\cos\theta \mp 1.\sin\theta \text{ and } x(\sqrt{-1}) \mp 1.y$$

into $\qquad +1.\cos\theta \mp 1.\sin\theta$ and $+1.x \mp 1.y$,

and consequently the equations

$$-1.\cos^2\theta - 1.\sin^2\theta = -1 \text{ and } -x^2-y^2 = -1$$

into $\quad +1.\cos^2\theta - 1.\sin^2\theta = +1$ and $+x^2-y^2 = +1$ for values of $\cos\theta$ and x greater than unity; and for such values of θ, the equation $e^{-1.\theta} \times e^{+1.\theta} = +1$ is still true.

By this *tour de force*, we have again passed from the circle expressed by $x^2+y^2 = 1$ or $\cos^2\theta + \sin^2\theta = 1$, to the equilateral hyperbola expressed by $x^2-y^2 = 1$ or $\cos^2\theta - \sin^2\theta = 1$; that is to say, from circular to hyperbolical cosines and sines.

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* It is known that arithmetical and algebraical development give different results in many cases, but such that both are true.

[In the calculus of potential functions, the following substitutions are made :

$$+\mathrm{Cos}\,\theta = +\cos\theta\,.\,\sqrt{-1},$$
$$+\mathrm{Sin}\,\theta = (-\sqrt{-1})\sin\theta\,.\,\sqrt{-1} = +1\sin\theta, \text{ and}$$
$$-\mathrm{Sin}\,\theta = (+\sqrt{-1})\sin\theta\,.\,\sqrt{-1} = -1\sin\theta\,;$$ the two last being identical, and the first precisely equivalent to the product $(+\sqrt{-1})(-\sqrt{-1})\cos\theta = +1\cos\theta$ (See PEIRCE's Treatise on Curves, Functions and Forces, vol. ii, p. 28). This method is tantamount to first multiplying both squares in the original equation $x^2+y^2=1$ by -1, and subsequently $-x^2$ again by that factor, making $x^2-y^2=1$.]

(27). From the researches in the three preceding paragraphs, it appears that the two characteristic properties of the symbol $\sqrt{-1}$, namely, its statical property or passive signification (as effect), by which it coincides with the principle of perpendicularity in being zero or neutral upon the primitive axis; and its dynamical property or active signification (as cause), by which its square produces negative unity, suffice to a complete and logical interpretation of the course of the quadratic equation $y^2=1-x^2$ for all positive and negative values of x. The alleged discrepancy between the hyperbola and circle with respect to the interpretation here contended for, is now shown to have no existence; the imaginary exponential is now geometrized; the mystery about imaginary arcs, it is hoped, is cleared up; and the truth of the relation in question, in all its broad generality, is insisted on, and believed to be firmly demonstrated.

(28). We may now review the origin and signification of the sign $\sqrt{(-1)}$. The question is, Given the signification and origin of the quantity $-1\,.\,1_{,,}$ to find, first, that quantity which, when multiplied by itself, shall be equal to the given quantity $-1\,.\,1_{,,}$ or, in other words, to find the square root of -1; and, secondly, to determine the true import and value of such square root. Observe that all the quantities concerned are nothing but the measures of the results of certain operations, and therefore we have only to define the operations which will produce such results.

1° The signification of the quantity $-1\,.\,1_{,}$ is, that it is the measure of a unit of effect, produced by a unit of force of the first order in the unit of time, and of a negative quality, or such that it would cancel, neutralize, or destroy an equal positive effect : it stands like the measure of the operation of subtraction opposed to that of addition, and is geometrically represented by a linear unit drawn in the

opposite direction to the linear unit which measures one operation of addition.

2° The quantity -1.1_{ι} may originate in two ways: first, by direct negative transfer on the primitive axis, from zero through the unit of distance, when 1_{μ} is multiplicand and -1.1_{λ} multiplier; and, secondly, by semirevolution of the radius unity, when $+1.1_{r}.1_{\mu}$ is multiplicand and $-1.1_{\theta} = 1_{\pi} =$ arc 180° is multiplier, which is the general way.

3° To find the quantity which, multiplied by itself, shall produce -1.1_{ι}, we have but to proceed in accordance with the operation which produced the quantity -1.1_{ι} itself. As this result was produced by one uniform operation in the unit of time, two equal operations in the unit of time, each equal in extent to half the former single one, will evidently reproduce the same result, and answer to the operation of squaring, as well as the former did to the operation of multiplying once; that is, the multiplicand being $+1.1_{r}.1_{\mu}$ as before, let the multiplier be $1_{\alpha} =$ arc 90°, whence by two successive multiplications we get $1_{\alpha}.1_{r}.1_{\mu}$ and $1_{\alpha}^{2}.1_{r}.1_{\mu} = -1.1_{r}.1_{\mu}$, so that $1_{\alpha}.1_{r}.1_{\mu} = (-1)^{\frac{1}{2}}1_{r}.1_{\mu}$.

4° The import of the symbol $\sqrt{(-1)}$ thus obtained is twofold: first, as a coefficient of the unit radius when it forms a right angle with and above the primitive axis, it indicates the perpendicularity of that radius, in which position it has evidently, as the measure of an effect in general, at the same time the value zero on the primitive axis and the value positive unity on the perpendicular, which is its statical value, and signifies the result $(+\sqrt{-1}.1_{r}.1_{\mu})$ of the first multiplication by the quadrant; and, secondly, as a multiplier, it is equivalent to the right angle or the arc of 90°, which is its dynamical value, and signifies that a multiplication by the quadrant is to be performed; and consequently when the multiplicand is $+\sqrt{-1}.1_{r}.1_{\mu}$, it produces the result $-1.1_{r}.1_{\mu}$.

5° This import of the symbol $\sqrt{-1}$ applies to it when a coefficient of a straight line. In consequence of the heterogeneity between angular and linear dimension, an angle has naturally a similar relation to a line that the perpendicular has to the primitive axis of measurement, namely, the relation of zero to unity, and *vice versa*. The sign $\pm\sqrt{-1}$ has the value zero, only because as the measure of a unit of operation it brings the line $+1.1_{\iota}$ into the position in which it becomes zero on the primitive axis; but by any such unit of operation in a plane, as multiplying the angle or arc θ by $\pm\sqrt{-1}$, that

angle is not brought into a position in which it becomes zero, since it is not and cannot be measured *upon* the primitive (or any other linear) axis, but only *from* such axis as its origin. Therefore although $\pm\sqrt{-1}$ may have a value zero with respect to lines, it does not follow that it must have such value with respect to arcs or angles; while its fundamental value as the measure of a unit operation renders it available as unity, and unity only, when a coefficient of angular dimension and of the arc its measure. Moreover in a plane there are *two* mutually perpendicular directions, each of efficient value in itself, but zero or neutral with respect to the other, and altogether subdivisible into *four* cardinal directions opposed to each other in pairs, corresponding to the four points of the compass, and appropriately designated by the four signs $+1$, $+\sqrt{-1}$, -1, and $-\sqrt{-1}$; but there are only *two* possible directions of angular movement or operation in the same plane, a direct or superior (positive), and an inverse or inferior (negative): of course, then, as it respects angular direction, the signs $\pm\sqrt{-1}$ must merge into ± 1. We should have then $\pm\sqrt{-1}.\theta = \pm 1.\theta$; but since either the arc equal to the radius unity, or the radius unity itself, may be made the multiplicand unit of space, and θ is an abstract number, we have always the double system of values $\pm\sqrt{-1}.\theta 1_{,} = \pm 1.\theta 1_{,}$ and $\pm\sqrt{-1}.\theta 1_r = \theta(\pm\sqrt{-1})1_r$, the former denoting an arc ($1_{,}$ being the unit arc), and the latter a line $\theta 1_{\prime}$ ($1_r = 1_{\prime}$) on the perpendicular axis.

6° As the coefficient of an exponent, the two values of the symbol $\sqrt{-1}$ have their significance:

First, its arithmetical value $\pm\sqrt{-1} = 0$, deduced from the primitive axis, renders the value $a^{\pm\sqrt{-1}.x} = a^{0.x} = 1$, which of course precludes geometrical involution, and consequently algebraical development, because $a^0.1^0_\lambda = a^0.0.1_{\prime} = 0$; but it allows the arithmetical involution to subsist, $a^0.1_{\prime} = 1.1_{\prime}$.

Secondly, its algebraical value $\pm\sqrt{-1} = \pm 1$ on the perpendicular axis renders the value

$$a^{\pm\sqrt{-1}.x} = a^{\pm\sqrt{-1}.x}.1_\lambda^{\pm\sqrt{-1}.x} = a^{\pm\sqrt{-1}.x} \times \pm\sqrt{-1}.x1_{\prime}$$
$$= 1 \times \pm\sqrt{-1}.x1_{\prime}$$ (because, in the result, the *exponent* must have its *arithmetical* value, which is zero, while the *coefficient* is *algebraical*), and thus allows the development to take place. Consequently in the function $a^{\pm\sqrt{-1}}$, the value zero is an *immediate* value of the exponent $\pm\sqrt{-1}$, which confines the function to its undeveloped but arithmetically involved value

$a^0 = 1$; while the value unity is *prospective*, under which value the development is allowed to proceed. When we regard an exponent as expressing the *number of times* an operation is to be performed, it must be an arithmetical number, among which $\pm\sqrt{-1} = 0$; but if the exponent express the *direction in space* in which the operation is to be performed, the symbol $\pm\sqrt{-1}$ necessarily conserves its general algebraical value.

(29). We may now say that we have arrested, and even bottled the shadowy imp, the mystical symbol of imaginarity, the impossible square root of one less than nothing. Like a notorious prototype, Asmodeus, our calculating imp performed many surprising and accommodating feats for the diversion and edification of his conjuring master; but unlike that prototype, his first advent hailed from the dim and distant fields of the vague unknown, and his journey has terminated where that of Asmodeus begun. Long viewed as an 'airy nothing,' he is from this day provided with 'a local habitation' — names enow, I ween, have hitherto been his acknowledged but his only dower. Fluttering in the regions of the twilight meridian, his haunts were closely beset by curious and indefatigable watchers, by some of whom he had at times been actually recognized upon his perch; but, anon, presto, he appeared suddenly in the east, in the north, in the west, and boxed the compass, or the contrary; and it was impossible to identify the true law of his being, or to remount to the source of his birth. Like the sad-voiced night-bird of the american forest, his notes told only in the obscure shades of the evening; and when looked upon under the light of open day, his appearance betrayed no intelligible correspondence with his mysterious function, which remained inscrutable, and even problematical, until all was again plunged in the medium of the *chiaroscuro*. Observe now his *double* janus-face, the fourfold source of that wondrous power which enables him incontinently to bridge the gulf of nothingness as though it were not there, then to lead off alone the long mazy dance of unity in all its varied steps, and at last to point his finger (index) significantly to the *primum locale* of the mighty musician who attunes the pythagorean harmony of the universe!

(30). The epithet *double janus-faced* is applied to the symbol $\sqrt{-1}$ in allusion to the *two* systems of *duplicate* values which it possesses, one system as an algebraical or mechanical, and the other as an arithmetical or numerical coefficient:

1° As a mechanical coefficient, it has, first, an *active* sense, as a multiplier or *cause*, under which signification it denotes a right angle as measured by the quadrant, and directs a unit operation *to be* performed; and, secondly, a *passive* sense, as a product or *effect*, under which signification it denotes the perpendicular radius unity, and records that a unit operation *has been* performed, and is measured by this radius.

2° As a numerical coefficient, it has at the same time the value zero on the primitive axis, and the value unity on the perpendicular axis, and is consequently a true coefficient of neutrality.

By its active or dynamical property, it subjects the unit measure of an operation successively to the four cardinal qualities of positivity, positive neutrality, negativity, and negative neutrality; and thereby enters into the genesis of equations and construction of curves as a fundamental and controlling element.

But as might have been anticipated from the mutual heterogeneity of the three elements, line, angle, and number, these elements comport themselves differently under the performance of a unit operation; and accordingly it has been found, first, that the straight line (under the emblem of a rigid movable radius) is capable of being placed, by a unit of operation expressed by $\pm\sqrt{-1}\,.\,1_\lambda$, in a position in which it has the simultaneous values zero and unity positive or negative; secondly, that the angle has no such capability, but that a unit of operation with respect to a given angle (expressed by $\pm\sqrt{-1}\,.\,\theta 1_r$) can only consist in the transfer of the multiplicand radius 1_r through that angle, in the direction in which it is measured by its arc $\theta\, 1_\theta$; and, thirdly, that a number expressed by $\pm\sqrt{-1}\,.\,n$ is a ratio susceptible of the duplex value $0\,.\,n$ and $\pm\sqrt{-1}\,.\,n$ proper; and when officiating as an exponent, the first value, being arithmetical, governs the undeveloped function $a^{\pm\sqrt{-1}.n} = a^0 = 1$, and may therefore be termed an *immediate* value; while the general value $\pm\sqrt{-1}$ is *prospective*, authorises the operation of geometrical involution ..... $a^{\pm\sqrt{-1}.n}.1_\lambda = \pm\sqrt{-1}\,.\,n1_{\prime\prime}$, and accompanies the process of algebraical development.

(31). It is concluded that the relation of agreement or coincidence which is known to exist between the square root of a negative quantity and the perpendicular radius of a circle is not an accidental one, but arises legitimately from principles and conditions which lie at the very foundation of the entire structure of the science of mathematical analysis. All numbers, ratios, or functions of x whatever, express or

imply the results of operations performed in space and time, and indeed at first only come into existence through the performance of such operations. The operation of multiplication may be looked upon as typical of all the others : the multiplier may have one of two forms, linear or angular ; and one of two qualities, direct or inverse, or some given combination of these two forms and qualities*. So long as the unit measure of the operation is fixed in quantity, that is, does not change by increase or decrease during the operation, we remain in the ordinary algebraical calculus ; and it is the variation of this measure during the actual performance of the operation itself, that gives rise to the fluxionary or differential and integral calculus. When the law of this variation is assigned, the result in space and time may be directly calculated ; and in this way the genesis of functions, and indeed the entire subject of mathematical development, both algebraical and transcendental (be the indices whole or fractional, positive or negative, real or imaginary), have an explanation in the calculus of operations, which achieves for the coefficients of Taylor's series that office which is performed by the calculus of permutations for the coefficients of equations : indeed the entire series may be constructed upon the blackboard, the effect of each term, and of each letter and sign in each term, having its distinct position in the picture. This calculus also comes in for a share of application to the theory of equations ; to the genesis and development of logarithmic, circular, and elliptic functions ; and applies

* Algebraists have uniformly proceeded as though impressed with a notion that products of two and of three factors, which of course imply the square and cube of the linear unit, must *necessarily* represent surfaces and solids. The principles and method of the calculus of operations readily explain the genesis of surfaces and solids by the particular manner in which the operations of squaring and cubing the linear unit are conducted (See Calculus of Operations, chapter ii, n° 8), but which is inapplicable beyond the third dimension ; while at the same time it is the distinguishing characteristic of those principles and that method to explain the operation of geometrical involution in a manner sufficiently general to apply to all dimensions whatever. The insufficiency of the notion referred to, and the inconvenience it has occasioned to analysts, appear abundantly in the abortive attempts hitherto made at the interpretation of quadratic equations whose righthand member remains negative after completing the square. This absolute term, viewed as a *negative surface*, can have no square root, that is, no single factor which, by two operations under the same law, can generate such surface ; but when regarded (as it should always be) in the more general aspec of a *negative line*, the single factor and its mode of operation are known.

directly, and with immediate elucidation, to the method of transversals in geometry. But this much will more than amply suffice for the announcement of pretensions confessedly somewhat extravagant; and further operations must now be suspended, until time sufficient shall have elapsed for the formation of opinions as to the validity of the principle I have proposed to adopt, so far at least as to absolve the doubt whether the calculus of operations, in the form in which I have attempted to present it, is *really* entitled to the appellation of A METHOD.

Prof. PEIRCE regretted that the time afforded Mr. Paterson was too short to explain the result of the researches of so many years of intense study. This was the first serious attempt to explain *the square root of minus unity* by perpendicularity. It was indeed a sublime attempt, which would eventually bear fruit of the most precious kind to all who were interested in tracing the effects which occur in the universe to those true laws by which thay were governed. He could not say enough in commendation of the result which the researches of Mr. P. had led him to, and he regretted that the time of the Section was so limited.

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2. ON A PROBLEM IN THE DOCTRINE OF CHANCES. By Professor B. PEIRCE.

[*Not received.*]

II. MECHANICS.

3. ON THE PROPER MEASURE OF MECHANICAL FORCE. By Prof. J. H. COFFIN, of Lafayette College, Pa.

AT our meeting in Philadelphia three years ago, some views were advanced in regard to the proper measure of the force of bodies in motion, which were admitted to be at variance with the received opinions of philosophers on the subject, but which were claimed to be both correct philosophically, and in accordance with the expe-

rience of practical men. It was stated by the distinguished professor who advocated those views, that if a globule of mercury were let fall upon the platform upon which he was standing, the force with which it would strike the platform would be proportional, not to its velocity simply, as had been usually supposed and maintained, but to the *square* of its velocity; and that this was the general law in regard to the force of moving bodies, viz. that it varied as the square of the velocity.

The same doctrine was repeated substantially at our last meeting in New-Haven, limited, perhaps, however (for I am not certain), to what was denominated its 'working power;' that is, the product of the space into the mass. It was said, in illustration, that the force of a ball shot through a series of parallel homogeneous plates would be properly measured by the number of plates perforated, and that this was shown by experiment to be proportional to the square of the velocity of the ball.

Now in relation to the remarks at the Philadelphia meeting, it is surely not to the credit of science, to be obliged to have recourse to the judgment of mere practical men, for the determination of a question so clearly within its own province; and meeting together, as we profess to do, in search of truth, I feel assured that it will not be considered discourteous in me, if I take conservative ground, and call in question the correctness of the alleged law, and the propriety of the measure of force from which it has been deduced, or at least state what I conceive to be objections to it.

The question, it is well known, is not new. It was debated earnestly, and (as I have always supposed) settled by the philosophers of Europe more than a century ago. In that discussion, those who advocated the doctrine that the force varied as the square of the velocity appealed to experiments such as the following. Spheres of equal magnitude, but of different weights, being let fall into some soft plastic substance (as tallow or wax), from heights that were inversely as those weights, made pits of equal depths in that substance. In this case, it is evident that the product of the weights of the balls into the squares of their respective velocities were equal; and as the depths of the pits were equal, it was inferred that the forces were likewise equal. Again, four *similar* elastic springs will impel a body with only double the velocity that a single one will do; nine, with three times, etc.: i. e. the force (represented in this case by the number of springs) is proportional to the square of the velocity.

Now if force is to be estimated by the amount of work it does, it is important to notice that it may be employed to do quite different kinds of work; or rather, perhaps, to do its work under quite different conditions.

1st. It may be required to communicate motion to a body free to move; as when one ball strikes another at rest.

2d. It may be required to overcome a fixed obstacle; as, for example, to break an inflexible bar, or to produce a pressure.

3d. It may be required to penetrate space, when opposed by some retarding influence; as when an arrow is shot into the air.

In the first case, there is no question but that the 'work done' is proportional to the velocity simply, and not to its square. The only experiment, so far as I know, that would even seem to favor the counter hypothesis, is that of the springs mentioned above, and that in appearance only; for it is to be considered that the force exerted by each spring is diminished in the same ratio that the velocity of the body is increased, so that by using four springs instead of one, and thus communicating a double velocity to the body, each spring exerts but half the force of the single one; and we thus have simply a double force and a double velocity, conformably to the law.

On the second of the above modes of applying force, I do not recollect to have seen the results of any direct experiments, where the moving body was a solid, though I presume there are many such on record; for the experiments would be very simple, and eminently practical. It is easy, however, to ascertain the law approximately from the experiments which have been made on fluids. The table published by Mr. Rouse many years ago in vol. 51 of the Philosophical Transactions, showing the relation between the velocity and force of the wind, is still considered the best authority we have on that subject. It is used by Rear Admiral Beaufort, hydrographer to the British Admiralty; and also by Prof. A. D. Bache, superintendent of the United States Coast Survey, in his reduction of the Girard College observations on winds. Now in a fluid current, the quantity of matter, or, in other words, the number of particles, which passes any section, is proportional to the velocity; and if the force of each particle is also proportional to the velocity, the aggregate force must be proportional to the square of the velocity. But if, on the other hand, the force of each particle is proportional to the square of its velocity, the aggregate force must be as the cube.

For the purpose of comparison, that we may see which law ap-

proaches most nearly to the truth, I have prepared the following table, in which the first column shows the velocity of the wind in miles per hour; the second, the pressure in pounds per square foot; the third, the pressure computed on the supposition that the force of each particle varies simply as the velocity; and the fourth, the same on the supposition that the force of each particle varies as the square of the velocity.

Velocity in miles per hour.	Pressure as found by experiment.	Pressure computed on 1st supposition.	Pressure computed on 2d supposition.
1	0,005	0,005	0,005
2	0,020	0,020	0,040
3	0,044	0,045	0,135
4	0,079	0,080	0,320
5	0,123	0,125	0,625
10	0,492	0,500	5,000
15	1,107	1,125	16,875
20	1,968	2,000	40,000
23	2,604	2,645	60,835
25	3,075	3,125	78,125
30	4,429	4,500	135,000
35	6,027	6,125	214,375
40	7,873	8,000	320,000
45	9,963	10,125	655,625
50	12,300	12,500	625,000
60	17,715	18,000	1080,000
66	21,435	21,780	1437,480
80	31,490	32,000	2560,000
100	49,200	50,000	5000,000

It appears from this table that the force of each particle in producing pressure is much more nearly proportional to the velocity simply, than to its square; varying but slightly from the former, but greatly from the latter.

But it is chiefly in regard to work of the third kind, where the moving body is required to penetrate space when opposed by some retarding influence, that the controversy has arisen. With the exception of the experiment of the springs, which I have already disposed of, all that I have ever seen or heard quoted to prove that the force varies as the square of the velocity are of this kind.

Thus in the old experiment of the balls let fall into a plastic substance, the depth of the pit is nothing more than the space penetrated by the body under the retarding influence exerted by the substance into which it falls; just as when an arrow is shot into the air, the height to which it rises is the space penetrated under the retarding influence of gravitation.

So also in the case of the parallel plates, quoted at the New-Haven meeting, the number of plates measures the space, and the resistance of each plate constitutes the retarding influence.

The case of water power, as applied to the grinding of grain, was also referred to at the New-Haven meeting; where it is evident that the number of revolutions of the millstone measures the space, and the friction of the grain between the stones constitutes the chief retarding influence.

And so we might go on showing that all the experiments resolve themselves into cases of retarded motion; and if the resistance be constant, as it is approximately in all such experiments, the motion is uniformly retarded. Now it is one of the first truths that we learn in mechanics, that in uniformly retarded motion, the space is proportional to the square of the velocity; so that if mere space in such circumstances, without reference to time, be the proper measure of the force of the moving body, there is no doubt but that the latter varies nearly as the square of the velocity. But there seem to be several serious objections to its being regarded as the proper measure.

1st. It gives results at variance with those obtained by the two other modes I have mentioned, which are, to say the least, quite as philosophical and worthy of credit.

2d. It introduces a new and very uncertain element, and one which, philosophically regarded, seems to me quite foreign to the question, and to serve only to make it more obscure and complicated, viz. the medium or influence which retards the motion. If in discharging a cannon we measure the expansive force of the gunpowder by the *velocity* given to the ball, or by the *pressure* upon the sides and breech of the gun, all is simple, and the results are harmonious; but if we attempt to measure it by the *distance* to which the ball is thrown, we must take into account all the resistances that it will have to encounter on its way, which may be uniform, or may vary according to any law whatever, or to no law at all. It is as though we should undertake to measure the capacity of a pipe for discharging fluid, by first perforating it longitudinally with a series of holes, so as to produce a leakage, and then inquiring how far a fluid, injected so as to fill one end, would flow before it should all leak out. If the diameter of the pipe and the rate of leakage were constant, this distance, like the analogous one in the case of force, would be proportional to the square of the velocity with which the fluid was injected, whereas the true measure of discharge we know to be pro-

portional to the velocity simply. Just so in all the experiments above referred to, the retarding influence constitutes a '*leakage*,' as it were, of the force, which gradually wastes it away till the whole is destroyed.

3d. It would seem that in estimating force by the amount of work done, we ought to take into account the time employed in doing it. Thus in the experiment of the parallel plates, by doubling the velocity of the ball, we shall perforate four times as many plates, but it will require twice as long a time to do it in, so that in reality the force should be considered as only doubled; and so in all the other experiments, if we divide the space by the time, and consider the quotient as the measure of the force, we shall find it to be proportional to the velocity simply, and not to its square. Thus understood, the three measures all harmonize; and the true doctrine seems to be, that the *absolute force* of a body in motion varies as the velocity simply; but opposed as it is in many of its practical applications by friction and other retarding influences, its *available working effect* in these cases varies more nearly as the square of the velocity.

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4. On the Occurrence of Placid Waters in the midst of large Areas where Waves are constantly breaking. By Prof. E. N. Horsford, of Harvard.

Professor Horsford said he had noticed frequently that there were spaces of some extent in places where the waves broke, which were very smooth; that though the swell or rise and fall of the water was just as great, yet there was no breaking of the waves, no white crest or comb. He believed that these smooth spots were occasioned by oil or oleaginous matter, which had accidentally happened to be spread on the surface at such places. To test this, he had, himself, when there was quite a stiff breeze, with waves on the surface of the water, which broke with considerable of a comb or crest, emptied a vial of oil, on the water, from a boat. The effect was instantly seen. As far as the oil spread, the water was smooth, and the waves did not break; and what was very curious, the oil spread over the surface almost as rapidly to windward as it did to leeward. He had, therefore, inclined to the conclusion that the smooth spaces

which might be observed in the midst of places where waves broke, were owing to the presence of oil, which might come either from decaying fish, or some other substance from which oil exuded.

Captain WILKES confirmed the statement and observations made by Prof. HORSFORD. He cited an instance where he had seen the same effects in a violent storm off the Cape of Good Hope, from the leakage of a whale ship. He stated it was very curious to observe over what a great extent a small quantity of oil would produce the effect spoken of.

Professor HENRY stated that almost every one knew the anecdote of Franklin stilling the sea, to the astonishment of the uninitiated, by stretching his cane over the side of the ship, the cane having a small vial of oil in the end of it. The subject was not new. It had been investigated very fully some twenty years ago, by order of the Dutch Government, and was published in the Annales de Chimie.

The philosophy of the phenomena was simple, though not given in the works on natural philosophy. It is this : When oil is placed on water, it has greater attraction for the water than for itself; while with water it is different, for it has greater attraction for itself than it has for the oil. If you attempt to separate the two by a disk placed on the surface of water which oil has covered, the break is not between the oil and water, but between oil and oil. He further stated that he had made some investigations to find out the thickness of the film of oil which spread over the surface of water; and for that purpose, he had spent a whole month in blowing soap bubbles. He thought the stillness of the waves was owing to the lubrication of their surface by the oil.

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5. ON THE PROPER GEOMETRICAL FORM OF THE MOULD-BOARD OF THE PLOUGH. By Rev. CHARLES HACKLEY, of Columbia College, New-York.

[*Not received.*]

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6. On the use of Air as a medium for conveying Mechanical Power. By Lieut. E. B. Hunt, U. S. Engineers.

Mechanical power applied to machinery plays so great a part in giving form and organization to civilized society, that the study of its sources, modifications and applications is full of import to all the material and spiritual interests of man. A large portion of our fellow-men live where they live, and do what they do, because of special facilities for wind, water or steam power furnished by the locality of their abode. Mechanical power is among the chief elements of national wealth, and is indeed indispensable to that cultivated leisure from which enlarged civilization springs. England owes her commanding position, not less to the great natural provision for mechanical power within her borders, than to that vigor of intellect and muscle which has raised myriads of iron arms to do her bidding. Mechanical power then forms, in civilized communities, a property of great value, subject, like all other property, to the principles of political economy.

Power, when cheaply available, creates the manufactures best adapted to its use. Moreover if power hitherto unavailable be made cheaply serviceable, new arts and manufactures will be established on spots till now unfitted for such a purpose. The chief purpose of this communication is to bring forward a mode of making available much power hitherto unserviceable, and of modifying the existing applications of mechanical power.

Elastic fluids give back, in expanding, most of the power requisite for effecting their compression. If we expend force in compressing air, this force may be recovered, and used at any desired point of the space occupied by the condensed air. By giving to this space the form of a long tube or passage way, the air may be condensed at one end, and used in driving an engine at the other end, just as steam drives a steam engine. Both theory and experience prove, that in such a case, the rapidity of flow will be very great. Power may thus be transferred through this atmospheric medium, from a point where it is not available, to one where it will fulfil all desired conditions. The only losses are those due to passive resistances of machinery, to cooling of the air heated by compression, and to its friction on the tube. The inertia of the flowing column would preserve that force

expended in giving the motion, acting as in the hydraulic ram. If the tube be made large, and the distance of transfer be not too great, all these losses will be very small. Steam might be used in this way, except that the cooling consequent on giving great length to the induction pipe would be very great, causing much loss of tension or condensation.

Again, force may be expended in forming a vacuum in a tube leading to the place where it is desired to apply any hitherto unavailable power. A sustained vacuum in a city would be a capital fortune. By its aid, atmospheric pressure might be made an exhaustless source of power. A piston, with the air on one side and a total or partial vacuum on the other, would act like that of a steam engine.

There are many places where water-power runs to waste, from the locality not being such as to permit its direct application. In some of these cases, the lost power may be very advantageously applied to compressing air, or forming a vacuum in a tube leading to an engine or engines placed where it is desired to use the power. This locality can undoubtedly be between one and two miles from the waterfall, without very great loss. By means of branch tubes, the power may be distributed to various points within the circle of two miles.

There is one remarkably favorable locality for applying the principles now stated. In the city of Rochester, the Genesee river has a vertical fall of about 100 feet. A dam just above the falls diverts the river into a canal, from which all the Rochester mills draw their supply of water. None of these use more than a third of the entire fall, and the water is then discharged directly over a precipice into the deep gulph below. Thus a fall of the entire Genesee through 60 or 70 feet, in the very heart of the city, is lost for mechanical purposes. The ravine is so bold and gulph-like in character, that mills can never be well placed for using this waste power directly. There is on the lower level just about the space required for placing the air-condensing or exhausting machinery, necessary for utilizing the waste power. The water can be taken from the tails of the present mills, led through tunnel and shaft conduits, and then applied to pumps for condensing or exhausting the air of tubes leading up the bank, and across the upper plateau, to those localities best adapted to mills and machinery. This tube could perhaps best be formed by driving shafts and tunnels, of about six feet square section, into the soft shale forming the bank and substrata, a construction which would be entirely durable; or by opening a trench, a brick or concrete

culvert lined with an air-tight plastering could be quite cheaply made, of such size that the velocity of air within need never be very great. If the trench be carefully filled, leakage of air may be made insensible. Branch pipes of cast iron will suffice for distributing the power from the main channels.

In the power-using establishments drawing from the common air or vacuum reservoir, the machinery required will be identical with that of the steam engine, from the induction pipe onwards, almost without modification. An examination of the steam-generating and steam-using portions of the steam engine will show that the connecting induction pipe exactly fills the place of the proposed power mains. The small size of this pipe in common engines shows that great powers may be passed through narrow tubes for some distance, with but slight loss. Indeed steam could be sent through tubes a mile long without much loss of tension, except for its high heat and liability to condensation; a difficulty not existing in the case of air.

The machinery best fitted for using the water power in this project would seem to be the water pressure engine. Two cylinders of equal length, placed horizontally end to end, would serve, one for the water, and the other for the air cylinder. Two pistons, one in each, and both on a common rod, would receive and use the pressures due to the head and expenditure of water. To this rod a fly wheel should be attached. The number of these engines would depend on the amount of water used.

It will be seen on examination of this project, that the entire water power of the Genesee, swelled, as it soon will be, by the surplus waters of the enlarged Erie canal, may be utilized and distributed through the city of Rochester, by constructions, no portion of which is untried, and no feature of which is particularly expensive. The good qualities of steam power can be secured, without expense for fuel, and with but slight cost for maintenance. The parts proposed may be made very durable, and wear of machinery reduced to a trifle. The consequences of increasing threefold the mechanical power of that fine city may be in part foreseen. The boundless agricultural resources of the famed Genesee valley will call into active requisition all the manufacturing capacities thus created. When the condition of the country permits manufactures again to prosper, Rochester will be able to start in this career with eminent advantages.

After having matured these ideas, I found that Papin, the ingenious inventor of the digester, &c., had conceived the same plan, and made some unsuccessful experiments in its application. He found the effect much less than he had expected. Though I have not met with any accurate account of his trials, I am convinced that he made his tubes too small, and neglected the friction of the air against their sides. Weisbach gives the expression for the resistance offered by friction to the flow of air in tubes,

$$h_n = 0{,}024 \frac{l}{d} \cdot \frac{v^2}{2g};$$

in which h_n is the height of a column of air equal to the resistance; 0,024, a constant derived from numerous experiments; l, the length of the tube; d, its diameter; v, the velocity of flow; and g, the force of gravity. Now as for a given discharge the velocity is inversely as the section, or as $\frac{1}{d^2}$, it follows that h_n varies as $\frac{1}{d^5}$; which shows how rapidly the friction may be diminished by increasing the diameter or section.

But the atmospheric railways have clearly shown the possibility of rapidly forming a vacuum at the distance of near two miles, without delay or loss of power. No sensible difference could be observed in simultaneous readings of two barometer gages at the ends of a nine-inch pipe, ½ mile long, at Wormholt Scrubs in England, when a vacuum of 18 inches was formed in one minute. On the Kingston and Dalkey road, 1¾ miles long, the pipe of 15 in. internal diameter was exhausted by the vacuum pump at Dalkey so rapidly that a vacuum was formed in the entire length from Kingston to Dalkey, equal to a column of mercury of 10 in. (⅓ atmosphere) in 0′ 56″;

.. .. of 15 in. (½ ") in 1′ 51″;

.. .. of 20 in. (⅔ ") in 3′ 30″.

After such results, it is unreasonable to suppose that any difficulty would be encountered in extending a vacuum or pressure through the proposed culverts, any distance within two miles from the pump. This, at least, is proved by these railways, unfortunately as they turned out for the stockholders. City gas distribution proves too that compressed gas may be distributed to great distances through narrow tubes.

The case of Rochester well represents one class of the applications which may be made of compressed air, in conveying waste power to points where it can be used. A rivulet from the mighty Niagara could thus be made to animate a city almost beyond its roar, if we can tolerate the thought of the King of Wonders becoming a Pegasus in harness.

One other application I will mention. In cities, it will permit the manufacture of power to be centralized in single power-making establishments; while by means of exhaustion or compression pipes, it can be distributed to large neighborhoods. All the steam-generating portions of steam engines may be replaced by central power factories, conducted on strictly economic principles. Thus the space, attendance, risk, and disagreeables of steam generating will be saved to stores and small manufacturing establishments; while all required power would be purchased from the power manufacturers, and distributed through the air mains just as in gas or water distribution. There would be a great economy in the balancing among many power consumers. The principles here are the same as in gas manufacture and distribution. It would be absurd for every body to make his own gas. The economy of power manufacture is scarcely less violated by each one making his own power. By using a common system of compression or vacuum tubes, this economy could best be consulted. Besides, power would then be used for hundreds of purposes where now it is prohibited by the necessity of boilers and engineers, the lack of room for their accommodation, and the risk incident to their use. It would be applied largely to lifting in stores and warehouses, making their upper stories almost as valuable as the lower. The machinery for thus applying it may be quite simple, and easily managed by an intelligent workman. The history of gas lighting and water supply may be re-enacted in that of power distribution. The simple principle in all this may be thus stated: Power is property, which may be manufactured and distributed to consumers at much lower rates than consumers can manufacture it for themselves.

III. PHYSICS.

A. MOLECULAR MECHANICS.

7. On the Permeability of Metals to Mercury. By Professor E. N. Horsford, of Harvard.

Daniel observed that bars of lead, tin, zinc, gold, and silver, became penetrated by mercury, when partially or wholly immersed in it. He noticed that mercury combined to form a crystallized amalgam with each of the first four metals, and, by the aid of heat, also with silver*.

Henry modified the experiment of Daniel with lead; giving to the bar the form of a syphon, one end only of which was immersed in the mercury. He discovered the remarkable fact that the mercury may not only be carried through the bar in this form, but that it will drop from the longer section of the bar; thus exhibiting the syphon experiment, employing a solid bar for the tube, and mercury for the liquid†.

I have repeated the experiments of Daniel and Henry, and have modified them in a variety of ways, to meet the inquiries suggested in the investigation of these phenomena; and I propose to give, in the following paper, the results at which I have arrived.

Experiments with Lead.

The bars employed by me, with a few exceptions for specified purposes, were cast in paper moulds surrounded by sand; of a dia-

* The production of amalgams by electrolysis is well known (Trans. Roy. Inst. vol. 1). Bottger prepared amalgams of barium, manganese, cobalt, nickel, zinc, silver, lead, copper and cadmium, by pouring salts of these metals upon sodium amalgam. Klauer observed the formation of calcium and magnesium amalgam, upon pouring salts of these metals upon potassium amalgam. Wollaston, Berzelius, Davy, Bergmann, Damour, Dobereiner, Serullas and Tennant have made various researches in this field.

† Pogg. lii. 187. Note. I met with the first exhibition of the experiment of Prof. Henry in the laboratory of Prof. Ten Eyck of Albany.

meter varying but slightly from 0,006^{mm}, and of variable lengths to suit the objects of experiment.

The following inquiries were submitted to experiment :

I. *Has the bar, saturated with mercury, increased specific gravity?*

Bars of lead, after standing in a cup of mercury until they had become saturated with the latter metal, were taken out and carefully scraped to remove the surface coat, and the specific gravity ascertained in the usual manner.

The following determinations were made :

Specific gravity of lead.		Specific gravity of lead saturated with mercury.	
11,431	Drawn bars.	11,409	Drawn bars.
11,405		11,426	
11,407		11,436	
		11,415	
11,414 average.		11,421 average.	
11,423	Cast bars.		
11,405			
11,387			
11,405 average.		11,464 Cast bar.	

They seem to indicate increased specific gravity.

The irregularity of these results led to an experiment to ascertain if there might be cavities in the bar. The specific gravity of mercury being greater than that of lead, as 13,575 (Fahrenheit) is to 11,448 (Berzelius), a bar containing cavities would have, when saturated with mercury, a higher specific gravity than a bar without cavities similarly saturated.

The following are the weights before and after being penetrated with mercury :

	Grammes of lead,	increased to	Increase.
I.	4,6342	4,6730 gr.	0,0388 gr.
II.	4,8403	4,9190 "	0,0787 "
III.	6,2051	6,3153 "	0,1102 "

The result was unsatisfactory : nearly equal weights of lead had absorbed weights of mercury differing from each other by a hundred per cent.

Upon examining the bars where in contact with the mercury, they were corroded, and lead was found dissolved in the mercury. More lead might have dissolved from one bar than the other.

Another experiment, made to determine the expansion of a bar of lead by the absorption of mercury, to which reference will be made below, confirms the opinion of higher specific gravity of the amalgam.

II. *What is the velocity of transmission of mercury through lead?*

It was observed by Prof. Henry, that the progress of mercury in the lead was more rapid in cast than in hammered lead. Upon noting the progress from day to day, most unexpected results have presented themselves. In a vertical bar, with the mercury at the bottom, the progress is at first rapid : it diminishes in velocity, however, from day to day, until, after several months, having reached the height of between six and seven inches, it is not one-thousandth as rapid as at the outset.

A hollow bar of lead, of ¼ inch calibre, was erected in a cup of mercury : the latter metal rose in six hours.......... 0,062 mm.

in six hours..........	0,062 mm.
in eighteen hours more,	0,008 "
in twelve days more ..	0,028 "
in eight do do ..	0,008 "
in eleven do do ..	0,010 "
in 112 do do ..	0,014 "
in 53 do do ..	0,007 "
in 53 do do ..	0,006 "
Total in 260 days	0,143 mm.*

In two cast bars, it rose somewhat more rapidly, and to a total greater height. In one (*a*) the velocities were as follows :

In 24 hours..............	0,085 mm.
in 24 hours more.........	0,010 "
in 16 days more	0,002 "
in 90 do do	0,054 "
in 53 do do	0,021 "
in 53 do do	0,006 "
in 53 do do	0,012 "
Total in 287 days	0,189 mm.

* The mercury rose, in a second similar hollow bar,

in 24 hours,	0,075
in 6 days more,	0,045
in 20 " "	0,014
in 53 " "	0,011
Total in 80 days,	0,145

The bar had previously been saturated with mercury, but had apparently lost most of it by evaporation.

In the other (b), which was composed of short pieces fused together, the mercury rose in 169 days.......... 0,224 mm.

	in 47 days more	0,011 "
	in 53 do do	0,015 "
Total	in 269 days	0,250 mm.

Placing these results side by side, we have the ascent of mercury in 24 hours, in drawn lead, 0,070 mm.
in cast lead, 0,085 "

The total height to which the mercury rose during the time of experiment, in drawn lead, = 0,143 mm.
in cast lead (a), = 0,189 "
in cast lead (b), = 0,250 "

The last result confirms a further remark of Prof. Henry, that the mercury follows the seams of a cast bar, rather than the more homogeneous portions. It is obviously a case of capillary attraction, the mercury ascending between the walls of narrow fissures.

III. *Does gravity influence the transmission of mercury?*

Mercury was presented at the top of a bar $0{,}80^{m}$ in length. Its descent was astonishingly rapid : in two hours it had penetrated 360^{mm}. The first quantity having all passed into the bar, it ceased to flow. Upon the addition of another portion, the flow was resumed. In less than two days, the mercury dropped from the bottom.

A syphon-shaped bar with the shorter leg out of the mercury, tnough it became saturated, discharged no mercury.

Gravitation evidently facilitates the transmission of the mercury when flowing from above downward. It of course opposes its flow from below upward.

IV. *Does the mercury which passes through the bar of lead contain the latter metal in solution?*

The drop presents a film upon its surface, which, as in a sack of very considerable tenacity, encases the purer metal. Upon volatilizing the mercury at a low heat, under a mass of cyanide of potassium, carbonate of soda and sand, there remained a button of lead.

V. *Is the lead contained in the drop derived from the end of the longer leg of the syphon, or from the interior of the bar as well as the end?*

In the latter case the interatomic spaces would be increased; and the mercury, under the influence of capillary attraction and gravitation, might be expected to flow faster. To ascertain if this might be, a syphon bar was arranged of diameter 0,06^{mm}, total length one decimetre. The amalgam dropped into a weighed porcelain cup, and was determined at intervals of ten days, and finally after a lapse of but four, emptying the cup after each weighing. The quantities that flowed through in the periods of ten days were:

	Grammes.		Grammes.
1st ten days ..	5,4169	6th ten days ..	18,9119
2d " ..	5,7906	7th " ..	24,6699
3d " ..	8,6281	8th " ..	29,5954
4th " ..	11,4976	9th " ..	34,6036
5th " ..	15,4280	10th " ..	40,0357
		Four days more gave ..	17,3920
		equal for ten days to ..	43,4806
		Total in 104 days..	210,9697

It will be observed that the quantity flowing through in the last four days of experiment is not as great a proportional increase as that of the preceding ten days.

A second experiment was made with another bar, employing the same mercury, now more or less saturated with lead. The length of the bar was 0,070^{mm}, and the diameter 0,006^{mm}. The first two weighings were at intervals of ten days; the remaining weighings, once in five days. There dropped in the

	Grammes.		Grammes.
1st & 2d five days ..	48,2735	9th five days	35,3460
3d & 4th five days..	66,5655	10th "	39,9694
5th five days	31,6409	11th "	36,0043
6th "	29,7585	12th "	31,2365
7th "	31,3590	13th "	31,5286
8th "	30,1640	14th "	31,9990
		Total in 70 days	443,8452.

The first suite of experiments led to the conclusion, that the increased flow of mercury in a given time was due to the increased porosity of the lead—to increased capillary attraction.

The second suite of experiments did not sustain this conclusion. In the first place, contrary to expectation, a new bar transmitted more mercury than the bar conceived to have become highly porous by use; and in the second place, the quantity transmitted in a given time soon attained a maximum, from which it varied but little to the close of experiment.

The 5th, 6th, 7th, 8th, 12th, 13th and 14th weighings gave quantities varying but little from each other. The 9th exceeded any of the preceding determinations so much, that I was led to a careful inspection of the circumstances attending the experiment.

I found the bar more deeply immersed in the mercury. Its position was maintained for the two succeeding experiments, and then changed to that occupied at first. These weighings led to the opinion that the cause of the discrepancy was the unequal absorbing surface to which the mercury had been exposed. More mercury had passed into and through the bar in one case than the other.

This explanation was confirmed by an especial experiment to ascertain,

VI. *What is the influence of the extent of absorbing surface exposed to the mercury?*

Two bars of equal length and diameter were taken. They were bent into syphons, and the shorter leg dipped in a solution of gutta percha in chloroform—a sort of collodion, which incrusted them with an impermeable envelope. After drying, the gutta percha cuticle was scraped from the end of one bar, and from the end and a nearly equal portion of the side of the other. The shorter legs of both were placed in the same cup of mercury, and the large legs in other weighed cups.

Two drops fell from the bar having the larger surface, before any fell from the other. After nine days, the quantities were weighed. Through the bar having the greater absorbing surface there had flowed .... 3,8902 grammes;
and through that having less, 2,1285 "

It might be supposed that the syphon action would be limited by the height to which the mercury rises in a vertical bar. An experiment was made to ascertain,

VII. *Whether the ascent of mercury be influenced by the vertical elevation of the summit of the syphon above the mercury, or the length of bar between the mercury and summit?*

A bar was saturated with mercury, and then bent into the form of a syphon; the shorter leg being 0,150^{mm}, the larger 0,800^{mm} in length. At the end of thirty-four days, there had appeared no drop. At this period the shorter leg was inclined at an angle of 15° to the horizon. In 129 days no amalgam had dropped from the longer leg. As the bar was saturated with mercury, the height to which the latter metal rose in the syphon could not be ascertained. It is evident nevertheless from this experiment and those made under inquiry II, that the progress of the mercury is so slow after having penetrated some 0,150 to 0,200^{mm} of mere length, that *length* influences more than vertical height.

From the results of the first and second series of experiments, arose naturally the inquiry,

VIII. *Does mercury saturated with lead flow through leaden bars?*

The following experiments were made :

1. Two syphon bars were placed in mercury that had once run through lead : in three days, drops fell from both.

2. Mercury, in which lead had been standing for months, and which was viscid from the presence of crystallized amalgam, was taken, and two bar syphons, one saturated with mercury and the other pure, were placed in it. In due time the amalgam fell from both.

3. Three syphons of nearly equal length were placed in a cup of mercury. In due time the amalgam dropped from all. In a few days the cup was emptied. As it ran through, it was received into a second cup, from which, when the first was emptied, it was poured back, to run through a second time, and a third, and so on.

The amalgam thus ran through some twelve or more times. It was saturated when it first came through, for it had every facility for acquiring the largest measure of lead it could hold. In this condition it ran repeatedly through the bars.

The quantity of liquid amalgam diminished; and there accumulated in the cup, at each end of the bar, crystallized amalgam. (The excess of mercury had evidently evaporated.)

Bars brittle when first withdrawn from the mercury, in time recovered their tenacity, and apparently with the loss of mercury by evaporation. This led to analyses in answer to the inquiry,

IX. *What is the constitution of the bar saturated with mercury, when in contact with the mercury, and also after long exposure to the atmosphere?*

An analysis of the saturated bar, the lead determined as sulphate, and the mercury as sulphide*, gave :

I. Of lead	96,32	per cent.
of mercury	3,63	
	99,95	

Another analysis of the same gave

II. Of lead	96,39	per cent.
leaving of mercury ..	3,62	
	100,01	

An analysis of the bar after seven months exposure to the atmosphere, gave

I. Of mercury.........	0,83	per cent.
leaving lead	99,17	
	100,00	

Another analysis gave

II. Of mercury.........	0,88	per cent.
of lead	99,22	
	100,00	

* An analysis of the saturated bar, by fusing under a mixture of carbonate of soda, cyanide of potassium, and sand, gave of

Lead - - -	96,53
Mercury - -	3,47
	100,00

Another gave of

Lead - - -	94,25
Mercury - -	5,75
	100,00

It was obvious that if the loss by vaporization in one case embraced all the mercury, it included some of the lead in the second.

The mercury was determined by treating the bar with diluted nitric acid, and stopping the action as soon as the mass assumed a globular form. By this process, the lead, being a more highly electro-positive body, was first dissolved; and there remained of the solid metal only so much as belongs to the liquid amalgam, which was found not to exceed two per cent.

The lead in the second analysis was determined as sulphate.

The drawn bar, which, when removed from the mercury, was so brittle as to be readily broken by an effort suddenly to bend it, after the partial loss of the mercury by volatilization as already remarked, recovered nearly, if not altogether, its original texture and toughness.

A cast bar, the surface of which was not scraped, after a little time, lost no more of its mercury, as the following weighings show:

Weight	4,0476 gr.	Difference.
20 days later,	3,9971 "	—0,0505 gr.
10 " "	3,9975 "	+0,0004 "
10 " "	3,9978 "	+0,0003 "

At first the mercury on the outside and ends was at the surface volatilized. The crystalline amalgam prevented, thereafter, the escape of the mercury.

X. *What is the constitution of the amalgam which flows through the bar?*

A determination by precipitation of the sulphate of lead from the nitric acid solution, gave, in 2,7348 gr. of amalgam,

0,1008 gr. of sulphate of lead,
= 2,52 per cent of lead;
leaving 97,48 per cent of mercury*.

An analysis of the solid crystalline amalgam which formed about the bar where in contact with mercury, gave

* By heating a portion of the liquid amalgam under carbonate of soda, cyanide of potassium, and sand, there was obtained,

Lead	1,98
Leaving mercury .	98,02
	100,00

This determination is liable to the criticism already made upon this mode of analysis. It has value as a qualitative result.

Of lead	36,03 per cent.
of mercury	63,97
	100,00

XI. *What change will the saturated bar experience, long in contact with mercury?*

A straight bar 0,040^{mm} long, placed erect in mercury, was soon saturated throughout. No sensible change in its texture took place, till, at the end of 194 days, the portion just below the summit began to enlarge and crack open, displaying, at the end of a few days, crystallized angles and surfaces in the interior. The crystallization continued for ninety days, when the observations were terminated.

All the vertical bars that from the commencement of experiment had remained standing in the mercury nearly equal periods, cracked open more or less throughout their entire length.

The syphons employed in experiments under inquiry (v), after having been withdrawn from the mercury forty-four days, on being returned, did not promptly permit the quicksilver to flow through; but after forty-one days, began to crack : twenty-six days later, the mercury resumed its flow.

The velocity of transmission was greatly diminished. In sixty days there fell only 11,4604 grammes. The texture of the bar, and the play of affinities had both changed.

XII. *Does the bar expand at once upon becoming saturated with mercury?*

A piece of $\frac{1}{4}$-inch lead tube was split open, flattened and scraped bright, and its length having been accurately ascertained (0,198^{mm}), mercury was spread over its entire upper surface, care being taken to avoid the points where admeasurement had been made, and where expansion, if it occurred, was to be observed.

When the mercury had penetrated to the lower surface of the bar, admeasurement was again made.

The bar had not perceptibly increased in length, nor did it in the first ten days after the saturation with the mercury.

Experiments with Tin.

The fact that tin is permeable to mercury, was noticed by Daniell. The following inquiries were submitted to experiment :

I. *Is the specific gravity increased by being saturated with mercury?*

The experiment was made with tin before it began to crystallize, and after the crystallization had apparently come to an end.

Specific gravity	of pure tin employed in the experiment,	7,29
.. ..	of tin saturated with mercury	7,50
.. ..	of tin and mercury (crystallized amalgam),	8,00

II. *What is the velocity of transmission of mercury through tin?*

The bars used in the following experiments were cast in paper moulds, surrounded by dry sand.

A bar 0,006^{mm} in diameter was placed in mercury having 0,185^{mm} above the surface of the liquid metal.

The mercury ascended as follows:

In the 1st day	0,010 mm.
2d day	0,010 "
3d day	0,010 "
4th and 5th days ...	0,022 "
6th day	0,013 "
7th day	0,013 "
8th day	0,013 "

At the end of six days the bar began to crack open at the bottom.

In fifteen days the mercury reached the top of the bar, 0,185^{mm}; making for the last seven days a velocity of 0,013$\frac{2}{7}$^{mm} per day.

The movement of the mercury in tin differs greatly from that of lead: in the latter, the progress is by a sort of inverse geometrical ratio; in the former, it is remarkably uniform.

A second bar was fitted to the top of the first, and secured in a tube by means of corks, so as to preserve the contact. The mercury continued to rise through several days, and attained a total increased elevation of 0,217^{mm}. Before the mercury had entirely ceased to ascend, the bar below opened into numerous fissures, and the entire column of amalgam became eminently brittle.

Crystallization arrests the play of affinities upon which the ascent of the mercury depends.

III. *Does tin permit the syphon action?*

A bar of 0,006^{mm} diameter, 0,185^{mm} total length, was bent into syphon form, and the shorter division placed in mercury. In fifteen days the mercury dropped from the bar: in two days more, the bar

broke of its own weight. The brittleness of the saturated tin bar, taken in connection with the more rapid crystallization, made it impossible to perform the serial experiments undertaken with lead.

IV. *Does mercury, saturated with lead, flow through tin bars?*

A syphon of 0,170^{mm} total length, was placed in a cup containing mercury that had run through lead. In due time the liquid issued from the longer leg of the syphon. Upon analysis, the liquid was found to consist of only tin and mercury; the lead had been left behind. In the bottom of the cup was found a crystallized amalgam of tin and lead.

V. *What is the constitution of the solid crystallized amalgam—the bar of tin saturated with mercury?*

Several determinations were made of both mercury and tin; the former as sulphide, the latter as stannic acid. The mercury determinations were uniformly too high, from the unavoidable presence of free sulphur. The tin analyses follow:

	Tin.	
100 parts of amalgam gave	82,9	per cent.
.. ..	82,3	"
.. ..	82,4	"
.. ..	82,1	"
.. ..	82,4	"
.. ..	82,9	"
.. ..	82,5	"
	577,5	
Average	82,5	per cent.
Leaving of mercury,	17,5	"

These results give very accurately the constitution ($Hg\,Sn_8$), as the following numbers show:

Hg	100,00	17,51	17,50
Sn_8	471,34	85,49	82,50
	571,34	100,00	100,00

The amalgam that flowed through gave

Tin	1,55	per cent.
Leaving mercury.	98,45	"
	100,00	

The amalgam that flowed through, leaving the lead behind, gave

Tin	1,73	per cent.
Mercury	98,27	"
	100,00	

VI. *The bar of tin, as it becomes saturated with mercury, begins, as remarked above, to crystallize.*

If at an early stage in this crystallization the bar is bent, the outside cracks off, revealing a pith as distinct as if it had been at first cast, and then a sheath cast around it.

If the crystallization be permitted to go on, the fissures penetrate to the centre of the bar. Daniell observed that a square bar split into triangular prisms, the separating fissures following diagonal planes. If the top and bottom of the bar were rightangled terminal planes, the crystallization freed a pyramid at either extreme.

The bar being irregularly cylindrical, the fissures were formed as in the case of the prism — along the lines of least resistance.

VII. *Does the mercury volatilize from the saturated tin bar?*

An affirmative reply might perhaps have been anticipated from the results with lead. In this, however, as in other respects, the tin and lead are greatly unlike.

A bar of tin saturated with mercury was weighed at intervals of ten days. Its weights were as follows :

First	5,2061	grammes.
After 10 days	5,2061	"
After 20 days	5,2061	"
After 30 days	5,2061	"

It crystallizes very soon after becoming saturated; and then, as in the case of the lead, volatilization ceases.

Experiments with Gold.

The progress of mercury in a bar of gold is exceedingly slow. This fact was observed by Daniell. Under favorable circumstances, in a strip of rolled American coin $0{,}00006^{mm}$ in thickness, the mercury rose $0{,}008^{mm}$ in a period of 240 days. The surface of the mercury around the gold was coated with a coherent solid amalgam.

Mercury coming in contact with gold, as is well known, rapidly combines with it. The depth to which the mercury penetrates seems

to be influenced by considerations something like those which prevail with lead.

Experiments with Silver.

The progress of mercury in silver is scarcely more rapid than in gold. It rose in a strip of American coin $0{,}00009^{mm}$ thick, but $0{,}0085^{mm}$ in 240 days.

The circumstance that both the above metals were rolled, and of course compressed, and the fact that both were alloys, doubtless impeded the flow of the mercury*.

Experiments with Zinc.

A bar of zinc was cast in a crooked glass tube, so as to possess, without bending, the requisite syphon form. Upon being placed in mercury, it rapidly dissolved all below the surface of the liquid metal. The mercury, however, penetrated $0{,}0065^{mm}$; and in this condition, withdrawn from the mercury, retained a semi-liquid drop at the end of the shorter leg, 250 days, that being the period of observation.

Experiments with Cadmium.

A syphon of cadmium was prepared in the manner of the zinc syphon. It dissolved rapidly in the mercury, but there appeared after some time an enlargement of the body of the bar, $0{,}006^{mm}$ from the end of the shorter leg, which resembled that in the bars of lead, except that it did not crack open.

Experiments with platinum, palladium, iron, copper, and brass, gave only negative results. The permeability of several of these metals to molten tin, gold, and silver; and of iron to molten copper, is well known.

SUMMARY OF RESULTS.

1. *The specific gravity of lead is increased* by saturation with mercury.

2. The velocity of mercury diminishes as the length of saturated bar increases, and in a kind of geometrical ratio.

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*American gold coin contains 900 parts gold and 100 parts silver and copper;
American silver coin, 900 " silver and 100 " copper.

3. The progress is more rapid in cast than in drawn lead.

4. The total height to which the mercury attains is greater in cast than in drawn lead.

5. Gravity facilitates the flow of mercury from above downward.

6. The mercury which passes through a syphon-shaped bar of lead, contains lead in solution.

7. This lead is derived from the interior of the bar.

8. After the transmission of a certain amount of mercury, and the return of this mercury to be passed again, the amount transmitted in a given time attains a maximum.

9. The amount passed in given time, with a given length of the shorter leg of the syphon, is dependent on the absorbing surface exposed to the mercury.

10. The syphon action is limited by the same law that determines the height or length of bar through which mercury will pass.

11. Mercury saturated with lead passes through leaden bars.

12. The saturated bar is eminently brittle.

13. The saturated bar contains

3,55 per cent of mercury, and
96,45 per cent of lead.

14. The bar saturated with, and afterward withdrawn from the mercury, in seven months lost by atmospheric diffusion

2,75 per cent of mercury;
leaving only 0,80 per cent in the bar.

15. In this condition, the bar had nearly recovered its original texture.

16. After the loss of a certain amount by diffusion, the surface becomes coated with crystalline amalgam, and the diffusion ceases.

17. The liquid amalgam contains 2,52 per cent of lead.

18. The saturated bar long in contact with mercury assumes a crystalline texture, and cracks open.

19. After crystallization commences, the progress of the mercury is still more impeded.

20. The specific gravity of tin is increased by saturation with mercury.

21. The saturated bar soon opens by numerous fissures, presenting crystalline angles and surfaces.

22. The specific gravity of the crystallized amalgam is greater than that of the bar merely saturated with mercury.

23. The velocity of transmission of mercury through tin, is at first slower than that through lead; but it differs in being uniform, while the velocity in lead rapidly diminishes.

24. The syphon action in a tin bar cannot be long maintained, on account of the crystallization and consequent brittleness of the bar.

25. The crystalline amalgam has a constitution of ($Hg\,Sn_6$).

26. The liquid amalgam contains

98,55 per cent of mercury, to
1,45 per cent of tin.

27. The crystalline amalgam loses nothing by atmospheric diffusion.

28. Quicksilver permeates gold and silver, but very slowly.

29. Zinc and cadmium are permeable to mercury, but dissolve in it.

30. Iron, platinum, palladium, copper, and brass, are, at common temperatures, not permeable to mercury.

NOTE. I am indebted for most of the foregoing analyses to HOMER, JOHN HAGUE, WILLIAM HAGUE, MARINER, DWIGHT, WORCESTER and DEAN, pupils in my laboratory, who have kindly co-operated with me in promoting the research.

E. N. HORSFORD.

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8. ON COHESION. By Prof. JOSEPH HENRY, Secretary Smithsonian Institution.

[*Not received.*]

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9. ON THE PLASTICITY OF SULPHUR. By Prof. E. N. HORSFORD, of Harvard.

IT must have attracted the attention of many chemists, that when a nitric acid solution of copper, nickel or cobalt, with excess of acid, is precipitated by a current of hydrosulphuric acid, the precipitate takes on a singular plasticity and elasticity, resembling that of sulphur when suddenly cooled from an elevated temperature by immersion in water.

The cause of this peculiar property I have sought by experiment. Solutions of many metallic salts besides those above enumerated, including iron, zinc, and mercury, in excess of nitric acid, when treated with a current of hydrosulphuric acid, gave precipitates of the above character. It was obvious, therefore, that the kind of metal was of no importance, and I made the experiment with pure nitric acid. The acid was prepared by distilling from nitrate of potassa with English oil of vitriol : it was fuming, and of a deep green color, from the presence of hyponitric and nitrous acids.

I. Through the concentrated acid, a current of hydrosulphuric acid was passed. There followed promptly a precipitate, at first yellow, with immediate elevation of temperature to 95° C. This was sufficient to melt a portion of the sulphur precipitate, and impart to it an orange red or slightly brownish tint. The sulphur had the elasticity and plasticity above noticed.

II. Another portion of acid was diluted with an equal volume of water, and a current of hydrosulphuric acid passed through it. It yielded a similar result, without, however, any portion of it assuming the reddish tint.

III. The IId, diluted with an equal volume of water, making a solution of one of concentrated nitric acid to three of water, gave a similar result, though the sulphur was less decidedly plastic.

IV. The IIId solution, with an equal volume of water, making a solution of one of nitric acid to seven of water, was similarly treated, and gave a like result.

V. Contained one of acid to fifteen of water : it gave still a viscid precipitate.

VI. Containing one of acid to thirty-one of water, gave a precipitate, but it was not coherent.

From experiments I and II, it is inferred that the same conditions were present that occur in the fusion of sulphur. The source of heat is intense chemical action : in the first experiment, it was adequate to fuse considerable masses of the sulphur ; in the second experiment, enough at the instant of the precipitation to confer upon the sulphur its plasticity. In the experiment where an extremely diluted solution was employed, the heat was so feeble as to permit the sulphur to fall in the form of incoherent powder.

The sulphur recovered its brittleness only after several days, as has been remarked of fused sulphur cooled by immersion in water.

B. OPTICS.

10. On the Solar Light. By Daniel Vaughan, of Cincinnati.

[*Not received.*]

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11. Experimental Researches tending towards an Improvement of the Telescope.. By Prof. Alexander C. Twining.

If it be a fact in the prevailing classification of stars relatively to their orders of magnitude, that the light of each inferior order closely approximates to one half that of its next superior, we may ascertain, by comparing the performance of exquisitely wrought telescopes with the performance of the unassisted eye, whether any large fraction of light is lost in the artificial instrument. Having occasion, some years ago, to make this comparison for an immediate practical purpose, I found, from the best data then attainable, that the choicest glasses fell short, in their recorded penetrative power, by two or three entire orders of magnitude, of the performance to be expected; taking the eye as a standard, and supposing no other considerable loss of light than that by absorption and the surface reflections. This supposed progression of stellar light compared with magnitude was indeed too indefinitely established to form an element for any positive conclusion, especially one so broad as would be intimated by the foregoing comparison; namely, that *only one-eighth or tenth of the light is available for vision in the best achromatics* : still such a result might justly give additional interest to an experiment which, for analogous reasons, I was about to attempt.

The observations and trials following were made in March 1847, with a dollond of 3½ inches aperture, belonging to the apparatus of Middlebury College. By frequent previous use of the instrument, and some general tests, I had satisfied myself of its medium quality at least; and no fault in respect of achromaticity was observable. The object glass, when closely examined with a magnifier, exhibited many minute opaque spots dispersed in one of the lenses, but not occupying any considerable area in the aggregate.

Upon the wall of a dark room, an opened window of which looked out upon a rising ground some quarter of a mile distant, I placed, opposite the window, the title cut from a newspaper called the "VERMONT CHRONICLE," printed in distinct and sharp german text characters. The black strokes of the (m), or ordinary letters, not capitals, were one tenth of an inch broad and six tenths long. This title I illuminated at a slight angle by a covered light at a graduated distance, and viewed it at the same angle on the other side the perpendicular, to avoid interference, and yet obtain nearly a normal view. This last line of view would, when prolonged through the open window, strike a commodious situation upon the distant rising ground before mentioned.

Viewing the title through a tube whose aperture next the eye was 0,16^{in} in diameter, one could decide that its first distinct reading was practicable from a distance of thirteen feet, when the light had approached to eleven feet. The eye-piece, whose power had been previously ascertained to be 100, was now screwed upon its tube; and the spot of observation was transferred to the rising ground at 100 times the distance, or 1300 feet by measurement made beforehand. The night was calm and dark, and the open window afforded an unobstructed line of sight to the illuminated object, and at the same angle as the eye view. The circumstances, therefore, of this second view, especially the leading one of the *visual angle*, would be unchanged from the first, except as affected by the transit through thirteen hundred feet of air, and through the telescopic arrangement; exact equality of field being unimportant.

By signals, an assistant in the chamber was instructed when and by what degrees to move the light towards the object. So much beyond the expected amount of illumination was requisite, that the nearest extremity of the range was occupied, and the title was not yet legible. Apparently, however, the verge was just reached; and this impression was confirmed by a subsequent repetition of the observation, with some changes. The light was found at nine inches of distance by measurement, and, being carefully tested, exhibited an unchanged intensity compared with the beginning.

From the identity of the visual angle in the near and the distant view, it follows that any given portion of the field would cover the same or an equal area of the retina in both. The absolute illuminations of the object were, relatively, as the inverse squares of the distances 11 feet and 9 inches, or as 215 to unity. Of this, only the

ten-thousandth part would reach the object glass, by reason of the hundredfold distance. But, again, the area of the glass, compared with the aperture of the eye tube used for the near view, was in the duplicate ratio of $3\frac{1}{4}^{\text{in}}$ to $0{,}16^{\text{in}}$, or $412\frac{1}{2}$ times the greater; yet the entire pencils would enter the eye, being less than one-thirtieth of an inch in diameter. Supposing then one-fifth absorbed and reflected, the relative quantity of light in the distant view and the near was as $\frac{4}{5} \times \frac{215 \times 412{,}5}{10000}$ to unity, or as 7,1 to 1. Six-sevenths, at least, of the light was therefore either extinguished by interference of rays, or was ineffective, from some other circumstance, for vision. Indeed had the actual verge of distinct legibility been reached, a yet larger fractional loss would have been manifest; for the succeeding experiment evinced that a distinct legibility, under these circumstances, was attainable.

In this succeeding instance, the near view of 13 feet first gave distinct legibility to the unassisted eye through the aperture of $0{,}16^{\text{in}}$, at 9 feet distance of the light from the object. The latter were placed in close proximity, and the distant or telescopic reading was clear and perfect, notwithstanding a tremor from unstable atmospheric refraction. By signals the light was now put into a receding progress, until the limit of legibility was reached and passed. Indeed the recession was, in fact, continued to a point obviously more distant from that limit than in the first experiment; not wisely, perhaps, had it not been the expectation to renew and vary the observation. But although this expectation has not to this time been realized, the two, as they stand, suffice for the object of this paper. The final distance of the light proved to be $8{,}2^{\text{in}}$, which, compared with 9 feet, shows but little more than $\frac{8}{10}$ths of the final illumination in the first mentioned distant observation, and therefore adds no new degree of approximation to the ratio deduced from the former experiment, but is specially important in confirming it, in consequence of its having proceeded from clear legibility backward to obscurity, so as to be a counterpart to the other.

To explain the great loss of light, amounting in these experiments to $\frac{6}{7}$ths of the whole after proper allowance for absorption and reflection, three occasions of loss may be mentioned; of which, however, the third alone will be dwelt upon, as it embodies the practical issue towards which these experiments were directed. *First,* there might be named the possible extinction of light by interference of

rays between the object glass and focus, and in crossing at the focus. *Second*, a diffusion of light at the focus, arising from imperfections of workmanship and material. *Third*, the chromatic aberration of the eye-pieces in common use.

It is remarkable that the best optical authors treat of these eye-pieces—the positive and the negative—as *achromatic* (that of Ramsden partially, and that of Huyghens perfectly), without apprising the reader, or appearing themselves to be aware, that it is only in their capacity of field-glasses that this property can be justly ascribed to them. In bending the pencils or brushes of light, which would otherwise escape the eye, towards and into one space not larger than the pupil of the eye, color would of course be manifested laterally by any single lens. The combinations above spoken of correct this lateral imperfection—Ramsden's partially, and Huyghens's perfectly; but with respect to that convergent and divergent refraction which forms images, or reduces pencils to beams for ocular reception and refraction, not only there is no correction, but one lens accumulates its aberration or color upon the other. This is obvious from the fact that the more refrangible rays, which should be, for the purpose of correction by the eye lens, nearest that lens, are really farthest from it. On this account the light from absolute physical points of the telescopic focal image, which should be collected into absolute physical points on the retina, is in fact diffused into minute circles around them.

A physical point on the retina may be conceived of as any area within which the entire light is effective in a single direction without appreciable diminution. If the retina be a network of similar chords receiving impulses, and made vibratory, as the principle and basis of vision, then a physical point would be closely of a diameter equal to half the diameter of a circular section of one of the chords. A diffusion into any space much exceeding this would, of course, sensibly commingle directions and impair vision. In other words, *visual effect is dependent on concentration of light*. If physical points were mathematical points, the slightest diffusion would annihilate visual effect, by infinitely *diluting* the light; but, although not mathematical points, these may be so minute that the diffusion created by chromatic aberration of an eye-piece can enfeeble visual effect to an extent never yet suspected.

An illustration of these views, and a confirmation of the main fact alluded to, may be instanced in the performance of telescopes not

achromatic. One philosopher, at least, has expressed his surprise at the distinctness and visibility of images formed by object glasses of one kind of glass only. In his view, the explanation is found in a great superiority of certain rays of the spectrum above the rest in exciting vision. The true explanation, however, is doubtless supplied by the preceding fact or principle. Assume a crown glass object lens of three feet focal distance. The rays which would, by a uniformity of refractive index, be concentrated into a point, are, by chromatic aberration, disposed in numberless focal points all along an axial line, for, say one inch and a quarter. But, in traversing this last extent, the focus of the eye lens is, anywhere, in place for a certain small part of the rays to be collected at and very near to a physical point on the retina, while the mass are diffused all around. But the visual effect of concentrated light so vastly transcends that of diffused light, that even the small pencils referred to overcome the far greater balance that remains, and rise superior to their confusing and obliterating effect.

Employing the front or illuminating lens of a solar microscope as an object glass, and a single lens of 1,10 focal distance as an eye glass, I constituted a temporary telescope of uncorrected refraction. It had 4½ inches aperture, about 27 inches focal distance, and nearly 25 of magnifying power. With this planted on a window seat, I viewed against the sky the vane and dark cross work of a spire half a mile distant, illuminated by sunlight coming from behind my station. Pushing forward or drawing back my eye lens, I found the entire range of vision to be very nearly the computed axial extent of chromatic and spherical aberration combined. At and near the extremes, vision was of course indistinct; but for two-thirds of the range, there gleamed from the central bosom of the gorgeous cloud of commingling and ever-changing halos of dispersed light, a clear image of the vane and spire. The practical conclusion is irresistible: if rays insignificant in number compared with the mass, can, when concentrated, stimulate distinct vision in direct contrariety to antagonist rays in immensely greater numbers, then, conversely, the effect of diffused light in great quantities may be inferior to that of concentrated light in trifling quantities; or, *the quantity of light being given, a trifling diffusion may very perceptibly and injuriously enfeeble visual efficacy.*

This exhibits the gross and general issue; but to a specific and unhesitating conclusion, measured results were necessary. From the

investigation of *degrees* on this delicate topic one might shrink, unless supplied with the best arrangements from the hands of finished opticians. I in fact approached it with an illuminating apparatus mounted at a cabinet shop, and my *pseudomorphic* telescope fitted with eye-tubes and sliding orifices by a common tinman.

The illuminating apparatus was a prism set into an orifice in a window shutter of a large philosophical room, from which all light from other sources was excluded. The prism was movable around its own axis and one perpendicular to it, and, by total reflection, threw a beam, under proper changes of adjustment, upon a given locality at the further extremity of the chamber. Here was placed a clearly printed page of specimen type in ordinary roman letter, and all secondary illumination was cut off by screens properly placed. This page, of about two inches square, containing a passage from a latin author, was read at two feet distance by the naked eye through an aperture of $0,15^{in}$, and with the lowest illumination compatible with legibility. It was then inverted and read at twenty-five times the distance, or 50 feet, with a power of 25; and therefore under the same visual angle as at first. The focus of the eye-piece in this reading was always at the most illuminated or effective spot in the focal spectrum of the object glass; which was found to be $0,38^{in}$ from the red extremity of a spectrum $1,13^{in}$ long, including spherical aberration; which coincides exactly or nearly with the spot of maximum illumination in Sir David Brewster's figured spectrum, as found in Dr. Bache's edition of his Optics. A sliding orifice interposed between the focus and the object glass, varied the quantity of beam admitted to the eye, simply by being pushed forward or drawn backward. Not to multiply particulars, I may observe, that by experiments made on the 16th and 18th of October in the year first mentioned, the two actual quantities of light admitted to the eye in the distant or telescopic view were, respectively, $69\frac{1}{2}$ times and $82\frac{1}{4}$ times the same in the near view, or by the eye alone through the small aperture. As an average, we may take the number 76, and conclude that $\frac{75}{76}$th parts approximate to the light which was inefficacious, and $\frac{1}{76}$th part to that which was effective.

I pause here to say that whenever these experiments shall be repeated with means and arrangements more refined and complete, the ratio above given will of course be found only an approximation of the rudest description, because my opportunities partook of that character. In marking and clearing out the first path under such

circumstances, one who invites the severest scrutiny into his procedures, as to principle and a promise of utility, is entitled to expect also great indulgence in the range of unavoidable errors.

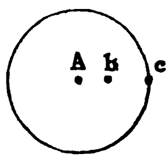
The illumination at that point of the solar spectrum where it is a maximum, appears to be 2,4 the average illumination throughout as modified by spherical aberration. One seventy-sixth part, therefore, of the light of a focal spectrum would emanate from the $\frac{1}{182.4}$th part of its total length at and near the spot of maximum illumination, if that alone were effective and the remainder lost. As the total length in this instance was 1,03 inches, all the effective light might emanate from points comprehended in a linear space of 0,0056^{in}, or 0,0028 in each direction from the focus of the eye lens. As the orifice which admitted the light was 0,64^{in} in diameter, and at the distance 6,70^{in} in front of the focus (or as 1 : 10,46), it appears that the diameter of the smallest circular space transmitting the same light was 0,00027^{in}. The focal distance of the eye lens was 1,10^{in}; from which we have the ratio of unity to 0,00025, or $\frac{1}{4000}$th, subtending of course an angle of $51\frac{1}{2}''$ of a degree.

It is desired now to compare this with the angular chromatic aberration of the eye-pieces in the dollond experimented with from the 1300 feet distance, as named in the beginning of this article. In that telescope, the extreme refractions by the negative eye-piece were at least $9\frac{1}{2}°$; but as the eye would seize upon the prominent rays, we may halve the aberration angle, and average in the whole pencil $3\frac{1}{6}°$ mean refraction. The eye-piece would therefore give a chromatic aberration of $\frac{1}{30}$th part of the same, or $380''$ of a degree; or 7,38 times as great in diameter as the circle of effective vision, and over 50 times as great in area. This is a species of conventional comparison, from which no absolute numerical result can be unerringly deduced; but it may suffice to evince the strong probability of a considerable injury by the employment of eye-pieces which are not properly and fully achromatic. The importance of such a conclusion, if correct, lies in this : that a prospect is thus opened of effecting as great penetrative or visual power in the telescope by the employment of eye-pieces truly achromatic, *as by enlarging the object glass with the ordinary eye-pieces.* The chromatic aberration of converging or diverging refractions, it may be easily made to appear, can be completely eliminated by two lenses of the same kind of glass; or, probably, by the two surfaces of a single lens of considerable thickness. The other or field aberration, it is true, will remain for objects

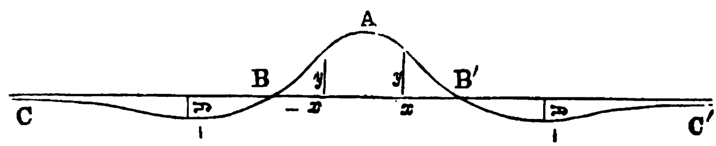
upon which the telescope is not truly centered; but, since the employment of regular equatorial motions, this last has ceased to possess its former importance in a scrutiny of minute points of sight.

Leaving here this subject to the candor and the skill of those who have leisure and means to prosecute such investigations through the last degrees of refinement, I proceed to discuss and supply one essential element of a complete investigation of this topic: I mean the *equation of the curve of visual effect*, and to hint at one mode of its employment.

INVESTIGATION.



Let A be a point on the retina, on which the visual effect of a given ray or element of light may be called unity (1). At certain distances, near and around A, we know that diffused light assists visual effect at A. A ray at b, for instance, would in some degree coincide in effect with one at A. But at a certain distance, as at c, light begins to be injurious; and, beyond that distance, becomes more and more so, until at a certain other distance the injury assumes a condition of decrease indefinitely continued, but never arriving at zero.



Let then CBAB′C′ be the curve representing the relation of visual effect to distance from the central point on the retina. Let y, one ordinate, represent that visual effect of a given ray at the co-ordinate distance x or $-x$ from the point. According to the preceding known facts, y is a maximum at A, or when x is 0. At a certain distance it is neutral, and the curve at B crosses the line of co-ordinates x. Thence y becomes negative; finds a maximum; and then diminishes without limit, but still negative.

The values of y for a given numerical value of x, whether positive or negative, are the same; and therefore the curve has two similar and equal branches, and y, when developed in terms of x, contains no uneven power. Let

$$y = \frac{A + Bx^2 + \dots . Zx^m}{A' + B'x^2 + \dots . Z'x^{m+1}},$$

in which the denominator must rise to one power at least of x^2 above the numerator, in order that y may be 0 when x is infinite. The four known requisites or conditions therefore confine the equation to

$$y = \frac{1-ax^2}{1\pm bx^2-cx^4};$$

putting y as 1 at the point A, or when x is 0; and a, b, and c as mere symbols of numerical value whose essential signs are all indicated.

This expression might appear to have more maxima and minima than three, which would be inadmissible. Its differential, however, taken at 0, conducts to the equation

$$x^2 = \frac{1}{a} \pm \sqrt{\frac{1}{a^2} + \frac{a\pm b}{ac}};$$

in which the radical part of x may always be greater than the rational, and give x^2 but two real values. These two, moreover, are numerically equal (positive and negative), and give the co-ordinate value of y negative.

The value of the constants in this equation must be determined by experiment. That of a is discovered by finding that position of the sliding orifice mentioned before in my experiments, at which the least amount of illumination gives visibility; and then $1-ax^2 = 0$. The value of b admits a close approximation by diminishing the orifice to an extreme, and illuminating intensely; in which case we have, very nearly, $y = \frac{1-ax^2}{1\pm bx^2}$. For c, more complication is requisite.

Again, this elementary equation conducts to an expression for the aggregate effect of light uniformly diffused over a circular space on the retina, whose diameter is $2x$, and whose elementary ray is m; for we have its differential

$$2\pi m \frac{x-ax^3}{1\pm bx^2+cx^4}dx,$$

where π represents the ratio 3,14159. This expression, having its coefficient a rational fraction, is of course integrable.

It would be useless to pursue this analytical branch of the subject at present, since the proper elementary experiments are incomplete. The expressions and applications which the foregoing equation involves will suggest themselves whenever their immediate occasion arises. Two remarks alone suggest themselves as specially applicable in the initial state of these researches.

First. The test or object of visibility should not be, as in the foregoing experiments, a printed page of coherent sentences, but a table of promiscuously composed numbers, and letters perhaps intermixed with the digits which they most nearly resemble. Different patterns of uniform size and appearance should be employed, at the election of an assistant, and without a clue given the operator as to the one on which he is experimenting. The readings should be as rapid as the assistant can record; and each pattern, and its record, should be filed together for subsequent attentive comparison. The reading should be made nearly at the verge of legibility; and the number of errors will afford a good numerical measure of—what might seem to admit no measure—*legibility.*

Second. In the dollond employed for the ascertainment of light lost and light made available, a negative eye-piece, magnifying 100 times, gave a diffusing effect upon the retina equal to a focal diffusion by the object glass over a space of fully $3\frac{3}{4}''$ in diameter; the eye being supposed, as before, to seize upon the prominent luminous rays, and bring the remainder to a focus too soon in part, and in part too remotely. Perhaps, therefore, one half the loss from the two combined was due to the eye-piece; and if this in fact was, as it appeared to be, $\frac{6}{7}$ths of the whole, *a truly achromatic eye lens would have made the effective light fourfold.* The same would be true of larger and finer instruments, under the same suppositions; except as affected by the ratio of aperture to focal distance, which, in my small instrument, was greater than is usual in the larger, and, according to my recollection (for I find no record of that circumstance), fell short but little of $\frac{1}{12}$th or $\frac{1}{13}$th. Other causes of loss, however, may perhaps divide with the two just mentioned.

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12. Relation of the Chemical Constitution of Bodies to Light. By Prof. E. N. Horsford, of Harvard.

Prof. Horsford called attention first to the well known facts that the color of the hair on animals varied, and was more intense on certain portions of the body. The metals also had colors which were affected by their composition. The change of their color in summer and winter was also a well known fact. He enumerated many metals

which changed their tints by the simple process of heating. These were phenomena which ought to be investigated by means of chemistry. The change of tint is without change in chemical composition. The law appears to be that metals pass from a lighter to a darker tint. The loss of water causes a change from a lighter to a darker tint. In charring wood, we have a change from a lighter to a darker tint. He illustrated on the blackboard that blackness was the natural color of all non-gaseous bodies; and he cited the series of compounds of gold, silver, nickel, platinum, tin, and other metals. He illustrated how the compounds of the several metals, as they became more divided in their molecular structure, varied. He exemplified them by the series of compounds of lead with oxygen, in which, as the oxygen prevailed, the colors became lighter. This was in keeping with discoveries made by Liebig, and other eminent chemists whom he named.

Dr. Draper had found the tints to vary in the order in which the metals had certain affinities, as in barium, strontium and calcium: he thought it was due to the metals which were at the base.

Blackness is appropriate to extreme dissolution; and, in this connection, it was worthy of remark that many nations had chosen that color to express extreme grief.

The conclusions of Prof. Horsford were, that the color of bodies depends upon the extent of the surface of their smaller particles, or groups of atoms. Transparency depends upon the arrangement of lesser atoms in certain order, constituting large groups. Whiteness depends upon such extent of surface of the groups of atoms as shall reflect all light, or upon such number of these plates produced by pulverizing transparent bodies as will reflect all the light. Blackness depends upon the subdivision of groups to such minuteness that they no longer reflect light, or, by producing interference, destroy it. Heat, by subdivision, causes darker shades. He also observed, in a note, that there seem to be successive scales of colors produced by heat.

Prof. Smith, of Louisiana, did not agree with Prof. Horsford in some of his conclusions, and showed that there were numerous exceptions in the mineral kingdom. There has recently been discovered the amorphous or black diamond. The diamond is generally supposed to be a clear, transparent substance; yet here was a specimen of a black variety, which was proved by the investigations of Dufresnoy to contain 98 per cent. of carbon. The color of this variety of diamond proceeded entirely from molecular structure.

13. Additional Facts respecting the Subjective Vision of the Arteries of the Retina. By Edward Hitchcock, Jr.

The experiment was first performed by Purkinje, and succeeds best when tried in the following manner : Let a person in a dark room, with one eye shaded, hold a common candle within two or three inches of the outer angle of the eye, and commence moving it upwards and downwards about the same number of inches, when a large picture of the arteries will show themselves at a short distance in front of the eye. Sometimes, besides the numerous ramifications, the insertion of the *arteria centralis retinæ* can be observed.

Prof. Wheatstone considers "that it is a shadow resulting from the obstruction of light by the bloodvessels spread over the retina."

Sir David Brewster offers a different explanation. He considers "that the light is propagated from the luminous image of the candle to other parts of the retina; and that though the retina in contact with the bloodvessels is sensible to direct light, it is insensible to propagated light, and therefore the bloodvessels are delineated in obscure lines."

The additional fact which I would mention, is that when an amount of light sufficient to perform this experiment is placed in a fixed position so near the eye, the pupil contracts; whereas, in this experiment, the pupil expands so widely and suddenly that it leaves the iris but a narrow belt. And if a second person watch the pupil of the experimenter, at the moment the image appears, the observer will notice a rapid and considerable enlargement of the pupil, which continues as long as the lamp is kept in motion.

Perhaps these few facts, as yet unmentioned, may give some light as to the true explanation of this phenomenon, which as yet seems unsatisfactorily accounted for.

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14. On a New Form of Microscope, with a New Mode of Measurement of Dimensions and Angles. By Professor J. Lawrence Smith, of the University of Louisiana.

The construction of this microscope is based upon the principle of placing the objective glasses beneath, instead of above the objects

to be examined. The tube in which the eyeglass is, is thereby 5° from the perpendicular, and the ray of light undergoes a deflection of 140° before entering the eye. The deflection is produced by a four-sided prism, with the angles 55°, 107°½, 52°½, 145°: the ray of light passing through the objective glass down, into the upper side of the prism, it penetrates and is subjected to two total reflections, and passes out of the fourth surface upwards, making an angle of 35° with the perpendicular. The eye regards, almost at the same moment, the object itself on the stage of the microscope, and the image of the same in the instrument; and here the advantage of the instrument is seen for chemical purposes, for which it was originally intended. It is, however, not confined to this; for owing to the convenience it affords for arranging the illumination, it is well adapted to much general use. The new plan of measurement is to introduce a micrometer into the tube of the microscope at any stage of the observations, by means of an arrangement placed so that the micrometer comes within the plane of the foci of all the eye-pieces used in the instrument. The method of measuring the angles of crystals is by having a graduated circle in the outer part of the tube of the microscope, and passing from the tube carrying the eye-pieces, which has a circular movement independent of that of the graduated circle. The manner of measuring the angles is as follows: Introduce the micrometer; turn the eye-piece until the lines on the micrometer are parallel to one side of the angle to be measured; then, leaving the eye-piece, turn the graduated circle until the index on the eye-piece is at zero; this done, turn the eye-piece until the lines on the micrometer are parallel with the other side of the angle to be measured, and, in regarding the circle, the degrees of the angle passed through will be seen. The method is convenient, and more precise than any known.

Prof. McCulloch suggested the application of Soleil's arrangement, especially in the examination of the phenomena of polarization; and he would beg leave to suggest to Prof. Smith an examination of sugar, as he lived in a vicinity where that article was manufactured. He said, with a few more additional improvements, which he suggested, the microscope would be almost perfect.

15. On the Apparent Motion of Figures of certain Colors. By Prof. E. Loomis.

At the New-Haven meeting of the American Association, I described some experiments relating to the apparent motion of a red worsted figure upon a green ground, or a green figure upon a red ground. I then stated that I had not been able to produce the same effects in a satisfactory manner with colored paper, and a member of the Association expressed the opinion that the *pile* of the worsted was essentially connected with the phenomena.

Although I had at that time performed some experiments which seemed inconsistent with the latter view, still as I had not succeeded to my satisfaction with certain materials, there was room for the supposition that the nature of the material might have some connection with the phenomenon. I therefore resolved to repeat the experiments under a greater variety of circumstances. This I have since accomplished, and the following are some of my results.

Experiment 1. I first tried the experiment in question with cotton thread. My earliest trials succeeded very imperfectly; but after a while I obtained a combination of red and green, and also a red and blue, which exhibited the phenomenon very nearly as well as the best specimen of worsted. I am persuaded that my first failures were due to my not having obtained exactly the proper combination of colors. The colors must be of the right shade and of the requisite intensity, and it is more difficult to obtain these in cotton than in worsted.

Exp. 2. I next tried the experiment with silk. Here again my first attempts were but partially successful; but I at length obtained combinations of red and green, as also of red and blue, which exhibited the phenomenon very handsomely, though perhaps not quite as well as the worsted. The difference I ascribe to the colors not being perfectly matched, and also to the lustre of the material, which decidedly interferes with the effect.

Exp. 3. I next tried the experiment with colored morocco, and succeeded without difficulty. The effect was scarcely, if at all, inferior to that produced with worsted.

Exp. 4. I next repeated my trials with colored paper, and, after some pains-taking, obtained specimens of red and blue, as also red and green, which exhibited the effect quite as well as the worsted.

I am satisfied that my former failures were due entirely to my not having been able to obtain colors of the proper shade and intensity. The color of paper is generally changed by moistening it; and many specimens, which do not succeed at all in this experiment when dry, succeed perfectly when moistened or oiled.

Exp. 5. I next tried the experiment with the colors of natural objects; and found that the red of certain flowers, combined with certain green leaves, exhibited the effect in question very handsomely. The scarlet flower of the verbena or geranium, combined with the green leaves of lettuce, succeeds beautifully. I had noticed this effect with flowers, previous to the meeting of the Association at New-Haven.

The preceding experiments with worsted, cotton, silk, leather, paper, and natural flowers and leaves, appear to me sufficiently varied to prove that the effect in question is entirely independent of the material employed. Color alone appears to be the essential circumstance. A particular shade of color is required, and also a certain intensity. A brilliant red, combined with its complementary color, will always produce the required effect. A lustre or gloss upon the surface interferes somewhat with the effect, but does not entirely destroy it. Such a result might have been anticipated, because the existence of gloss implies that foreign light (which of course is not homogeneous) is reflected from the surface in question, and mingles with the light emitted from the body under experiment. Of course the color of the body is changed, or at least rendered less brilliant by this mixture.

I arrive then at the conclusion that the wave-like motion which passes over a small red figure upon a green ground, when gently agitated, is an effect of brilliant complementary colors, and has no connexion with the nature of the material with which the color is associated.

The phenomenon to be explained in the experiment of the "dancing mice" is this: When a green figure or stripe is worked upon a red ground, and the card gently agitated, a shade of lighter green appears to spread over the whole figure, and overlap the surrounding red ground. A red stripe upon a green ground, when agitated, appears of a lighter red on each margin alternately, with a deep red wave oscillating back and forth at each motion of the card.

These phenomena are believed to involve the following principles:

1. The *continuance* of impressions on the retina. An impression made on the retina lasts for an appreciable interval. When a bright colored figure is agitated before the eye, it makes an impression upon a portion of the retina larger than that covered by the figure at rest. Thus a green figure upon a red ground, being agitated, appears to overlap the surrounding red ground.

2. There is a partial and transient *combination* of the complementary colors. While the impression of the central color remains upon the retina, the same portion of the retina, in consequence of the motion of the card, receives a new impression of the surrounding complementary color : in other words, two colors which are complementary to each other are impressed successively upon the same part of the retina, the new impression being made while the effect of the old one remains. These two colors partially combine to produce, not white light, but a much lighter shade of the primitive color. This explains the experiment with the green figure on a red ground; and it explains the lighter shade on the margin of the red figure upon a green ground. But the red stripe upon the green ground exhibits the remarkable peculiarity of a deep red wave oscillating from one side to the other of the stripe, at each motion of the card. In order to analyse this phenomenon, I tried the following experiment :

Exp. 6. When I used a broad red stripe between two green ones, the change of color was confined to the borders of the red, extending to a breadth of about a quarter of an inch ; a light stripe appearing on the upper side of the red when the figure was depressed, and on the lower side when the figure was elevated. No change of appearance could be observed in the intermediate red. Hence when the red stripe is only a half inch in breadth, there is the appearance of a red wave transferred from one edge to the opposite, at each motion of the card. If the red stripe does not exceed a quarter of an inch in breadth, the effect is much impaired ; and if the stripe be reduced to less than one-tenth of an inch, the wave-like appearance ceases entirely, and the red stripe appears constantly of a very pale red.

The following, then, is my opinion of the origin of this deep red wave. The red color appears to excite the retina more powerfully than the green, and its impression is more durable. When therefore I place a red stripe upon a green ground and agitate it, the effect of the green ground is confined to a narrow margin of the red stripe, not generally exceeding a quarter of an inch in breadth. Consequently if the red stripe is broad, say two or three inches, no peculiar

effect is produced upon the central part; but a band of pâle red, about a quarter of an inch in extent, is seen on the two opposite margins alternately. If the breadth of the red stripe does not exceed the tenth of an inch, its light is constantly blended with that of the surrounding green; that is, it appears constantly of a light shade, and there is no appearance of a dark wave passing over it. When the breadth of the stripe is about half an inch, the red and green colors are made to combine on the opposite margins alternately; the dark and the light bands succeed each other on the same part of the stripe, and the appearance is that of a wave-like motion.

Thus the two well-known principles, first, of the continuance of impressions on the retina, and secondly, that complementary colors combine to produce white light, appear to explain the essential circumstances of the phenomenon in question.

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16. Case of the Tertiary Rainbow. By C. Harswell, Esq.

[*Not received.*]

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17. The Bearing of some recent Microscopical Discoveries on the present Theories of Light. By Dr. W. I. Burnett, of Boston.

[*Not received.*]

D. THERMOTICS.

18. The Effect of Heat on the Perpendicularity of Bunker Hill Monument. By Prof. E. N. Horsford, of Harvard.

Soon after the pendulum was placed in Bunker Hill Monument, it was observed that the ball when at rest was not always over the same point in the floor. The careful consideration of all the conditions of this fact resulted in ascribing it to the unequal expansion of the sides of the monument, in consequence of unequal exposure to the sun.

A brief description of the present condition of the monument will aid in understanding the mode of observation pursued.

The obelisk, thirty feet square at the base, rises, gradually lessening, to a pyramidal summit two hundred and twenty-one feet. Within is a circular well, seven feet in diameter at the bottom, and five at the top, where it opens into a chamber or observatory. The chamber is approached by a winding stairway. In the centre of the roof of the chamber is an iron staple which was securely fixed at the time of placing the capstone. It served at first to support machinery for carrying visitors up and down. From this staple, which is over the centre of the open space or well, the pendulum is suspended by means of a screw clamp.

From a point in the floor directly below the index attached to the ball, circles were described and graduated, and radii drawn.

On the day following the graduation, the index was found to be on one side of the centre of the circle. As the screw clamp first employed did not admit of adjustment, a new apparatus, with the necessary modifications, was substituted, and the ball brought precisely over the centre of the graduated circle.

A few hours later, it was found out of the centre.

Upon observing more carefully, it was found during clear days that the motion of the ball in the morning was to the westward, at noon to the northwest, and at evening to the east. It was further observed that on days when the sun was obscured by clouds, that no motion of the ball or its index point occurred. It was still further observed on one occasion, during a sudden shower, accompanied with strong wind from the southeast, at about three o'clock in the afternoon, to move in the space of a very few minutes a quarter of an inch to the eastward. Observations at seven o'clock in the morning, at twelve o'clock at noon, and at seven o'clock in the afternoon, were recorded through several weeks, and no doubt remains that a cause coïncident with the sun in its progress produced the variation of the perpendicular in the monument.

A fact already hinted at, further confirmed this conclusion. The extreme departure of the ball from the centre was to the west of northwest; not to the north, as might at first glance be supposed. The explanation is found in the position of the monument. Its sides do not face the cardinal points, but are inclined about 20°. The expansion of a single side would produce inclination in a direction perpendicular to the side. The expansion of two adjacent sides

would produce inclination in the direction of the diagonal. In the morning the shaft is inclined to the westward. At noon it is inclined but little to the north of west. In the progress of the afternoon, it sweeps over twice the amount of movement in the morning; describing, in the twelve hours of observation, an arc of an ellipse.

During the night it sets back to the centre, and before seven o'clock in the morning, has already moved westward.

The greatest diameter of the irregular ellipse, described by the index in twenty-four hours, is ordinarily less than half an inch, while the least was less than a quarter of an inch.

It would not be difficult to find the expansion of the granite to which this movement of the ball corresponds. In the simpler case of a rectangular shaft, the departure of the ball from the centre would be the versed sine of an arc (the side of the shaft), of which the pendulum was the sine. The difference between the arc and sine would be the expansion of the granite.

The heat of the sun penetrates to but a moderate depth. This is evident from the prompt movement of the column when a shower falls only upon the more highly heated sides, and also from the ready change in inclination as the day advances.

The effects here observed, and which are now recorded from day to day, taken in connection with the meteorological record of Boston, Charlestown, and Cambridge, cannot fail to be of high interest.

The expansion of granite by heat had before been observed. Mr. Bond, the director of the Cambridge Observatory, noticed its effect on his transit instrument erected in the temporary establishment at the corner of Quincy and Harvard streets. The instrument rested on two granite pillars. In the morning of a clear day, his meridian mark on a distant hill would be found east of the meridian line as indicated by his instrument; at noon, or a little past, coïncident with it; and at evening west of it.

Engineers have observed it in long walls of masonry. It can scarcely be doubted that we have memorials of it in the ruins of Balbec and Pæstum, of Nimrod and Stonehenge; nor can we question that it has played a large part in the destruction of cliffs, or the splitting of mountain masses.

The mode of observation at the monument is this: On either side, about three-quarters of an inch from the centre, under the index of the ball, two slender needles have been driven into the floor, leaving not more than the sixteenth of an inch above. These are

made by pressure to pierce a card of thin drawing paper, which is kept from warping by slender bars of lead. When fixed, the north and south and east and west lines are transferred in pencil mark from the floor to the paper. After bringing the ball to rest, in which the observer is aided by a contrivance enabling him to steady his hands, a dot is made with a pencil immediately under the index point, which is about the sixteenth of an inch above the paper. At the close of the day, the card previously dated is removed, and another takes its place for the observations of the next day.

It is a grateful duty to state that the expense of the necessary fixtures at the monument for the pendulum experiment, of which advantage has been taken in the observations here referred to, has been incurred by the Massachusetts Charitable Mechanics' Association. The enlightened liberality of the directors of this association is only equalled by the generous and efficient coöperation of the officers of the Bunker Hill Monument Association.

B. ELECTRICITY AND MAGNETISM.

19. On the Theory of the so-called Imponderables. By Prof. Joseph Henry, of Washington.

Prof. Henry stated, that in studying the phenomena of matter, we commence with observing the action of masses upon each other, and from this we deduce laws. These, with regard to mechanical philosophy, are five in number, viz. the two laws of force, attraction and repulsion, varying with some function of the distance; and secondly, the three laws of motion, viz. the law of inertia, of the coexistence of motions, and of action and reaction. Of these laws we can give no explanation : they are at present considered as ultimate facts, to which all mechanical phenomena are referred, or from which they are deduced by logical inference. The existence of these laws, as has been said, is deduced from the phenomena of the operations of matter in masses ; but we apply them by analogy to the minute and invisible portions of matter which constitute the atoms or molecules of gases, and we find that the inferences from this assumption are borne out by the results of experience.

Indeed, the minutest portions of matter must be endowed with properties analogous to masses of the same kind of matter. An attempt has, however, been made by Boscovich to refer all the mechanical properties of matter to portions of space, filled with associated points, endowed with attracting and repelling forces, varying and alternating with changes of distances. In a communication, some time since, to the American Philosophical Society, I have shown, says Professor H., that this hypothesis, which is at the present time adopted by many, is insufficient to explain all the facts. Matter thus constituted would indeed exhibit the phenomena of elasticity, compressibility, porosity, affinity, etc.; but it would not exhibit an obedience to the three laws of motion, namely, inertia, the coexistence or composition of motions, and action and reaction. We must therefore superadd to the hypothetical points of Boscovich, these other conditions; but in so doing, we arrive at a constitution of matter precisely similar to that adopted by Newton, namely, a system of indivisible and indestructible atoms endowed with the essential properties of matter in masses. Indeed, this is the only hypothesis which we can adopt in strict accordance with analogy, reasoning from the known to the unknown.

Besides the phenomena of the action of invisible atoms of gases on each other, we have a large class known under the general name of the phenomena of the imponderables. This name has been given, because it is supposed that it is necessary to refer them to hypothetical fluids, not subjected to the ordinary laws of force and motion. The term *imponderable*, however, expresses a quality with reference to the constitution of such fluids, not warranted by the facts. A mass of air poised in air has no weight, and in this case may be considered imponderable. In the same way, if we suppose an elastic medium to pervade all space, any portion of this will be imponderable, even were our balances sufficiently delicate to detect its absolute weight. The existence of an elastic medium pervading all space is assumed in order that the phenomena of light, heat, electricity and magnetism may be brought within the category of the laws of force and motion, and that we may be able to apply the principles of analytical mechanics in the way of deducing consequences to be afterwards tested by an actual appeal to experiment. Without assumptions of this kind, it is impossible to arrive at the general expressions which constitute science in the proper sense of the term.

It is not necessary that a hypothesis be absolutely true, in order

that it may be adopted as an expression for a generalization for the purpose of explaining and predicting new phenomena : it is only necessary that it should be well conditioned in accordance with known mechanical principles. We have a remarkable instance of this in the Newtonian theory of emission of light. According to this, light is first considered as consisting of atoms of matter moving with immense velocity, but subject to mechanical laws. The inference from this assumption is, that meeting obliquely a reflecting surface, the atoms will rebound as would a perfectly elastic ball, making the angle of incidence equal to the angle of reflection. This fact being established by experiment, all the phenomena of reflected light are deduced mathematically as mechanical consequences from the primary assumption. Again, it is discovered that a ray of light, in entering obliquely a new medium, changes its direction ; and this is readily explained by adding to the previous hypothesis the second condition, that the atoms of light, like all other matter, are subject to attraction, and that they are, in consequence of this, accelerated or retarded in velocity at the moment of entering the new medium. From this assumption readily flows the law of the permanency of the ratio of the sine of the angle of refraction to that of incidence.

In the progress of discovery, it is further found that a ray of light is separated into different colors; and in order to explain this agreeably to the same analogies, we are obliged to admit that there are different kinds of atoms of light, with different properties, and moving with different velocities. Further, it is discovered that light, in passing by the edges of different bodies, produces fringes, and other phenomena known by the name of diffraction. To explain these, another supplementary hypothesis must be added, namely, that the atoms of light are alternately attracted and repelled by the variation in their distance from the solid body near which they pass. Another class of phenomena, denominated by Newton fits of easy refraction and easy reflection, induce the assumption that the atoms of light are not homogeneous in property on all sides, but that each possesses an attracting and repelling pole; and that in their passage through space, they are constantly revolving on axes perpendicular to the line joining their poles. Again, the discovery of Malus requires another supplementary hypothesis, in order to a mechanical conception of the phenomena first observed by him. To explain these, we must admit that the atoms of light possess different properties on different

sides, in addition to different properties at different ends. But now the original theory of emission, at first a simple mechanical conception, becomes so loaded with supplementary hypotheses, that as a whole it is unwieldy, and we are induced to look for some other possible hypothesis which shall equally well connect the phenomena in accordance with known mechanical principles, and not be subject to the same charge of complexity. Such an assumption is found in the present received undulatory theory of light.

In reviewing the foregoing sketch of the rise, growth, and abandonment of the theory of emission, we see that an hypothesis, though not absolutely true, may serve an important purpose in the way of the definite conception of old phenomena, and in the discovery and prediction of new; and indeed, in some cases, paradoxical as it may appear, a false hypothesis, from its ease of application, may be of more use than one which is absolutely true. Man, with his finite faculties, cannot hope in this life to arrive at a knowledge of absolute truth; and were the true theory of the universe, or, in other words, the precise mode in which Divine Wisdom operates in producing the phenomena of the material world, revealed to him, his mind would be unfitted for its reception : it would be too simple in its expression, and too general in its application, to be understood and applied by intellects like ours.

It may be asked why theories, so apparently different as those of emission and undulation, should both lead to the discovery of new truths? The answer is, that the former is involved in the latter, and that all the supplementary hypotheses we have mentioned have their representation in the different phases of wave-motion. Thus an undulation is reflected in the same manner as an elastic ball : a change in velocity also takes place in the undulation on entering a new medium; and the fits of Newton are represented by lengths of waves, and the polarization of Malus by transverse vibrations reduced to the same or parallel planes. The undulatory theory is a more general expression, and contains truths which are not to be logically deduced from the theory of emission. In order, however, that this theory may enable us to discover the greatest number of new phenomena, and assist us in ascertaining the more precise relations of known facts, it is necessary that all its parts should be definitely conditioned with reference to established mechanical principles. The phenomena of light and heat, and of chemical and phosphorogenic emanation from the sun, by strict analogy lead us to infer that something possessing

inertia and obedience generally to the laws of force and motion must exist between us and this luminary. All the phenomena are best explained and predicted by supposing this something to consist of an elastic medium, the atoms of which in a normal state are distributed uniformly through space, and retained in position by attracting and repelling forces. An ethereal medium, constituted in this manner, will admit of vibrations of different characters and of different forms: for example, if an impulse be given to an atom in a given direction, it will cause in succession a motion to be transmitted to the series of atoms which are found in the same line, and thus longitudinal undulations will be produced; also the motion of the atom to which the impulse is given will cause it to approach the atoms of the medium on the sides of the line just mentioned, and thus rows of atoms on all sides of the first row will be thrown into a state of transverse vibration. Similar systems of vibrations must also take place in air; but such is the constitution of the human ear, that it takes cognizance only of longitudinal vibrations; and such the function of the human eye, that it is only affected by transverse undulations. Besides these, there may be other vibrations compounded of the two; and in this way, other emanations than those which have yet been observed may be conceived to exist.

The science of electricity, as left by Cavendish and Æpinus, and as expounded by Hauy and Robison, was, next to astronomy, one of the most perfect of the physical sciences. All the known phenomena of statical electricity were referred to the mechanical action of two species of matter; the atoms of each being self-repellent, and attractive of the atoms of the other: one of these is called the electrical fluid, and the other ordinary matter. For the generalization of the same phenomena, Dufay assumed three principles: two species of electrical, and one of ordinary matter. From either of these mechanical conceptions could be deduced all the facts then known.

It would appear, however, that the tendency of the present day is to the accumulation of facts, rather than to their critical examination, or the discovery of general expressions by which to represent them. Electricity and magnetism at the present time consist of almost a chaos of isolated phenomena, which can scarcely be called scientific. Most of these, however, I am convinced, are capable of being referred to the theory of Franklin, or to that of Dufay, with the addition of a few supplementary hypotheses analogous to those which we have seen were added to the theory of emission. For example,

we shall be obliged to admit that in some cases inductive effects are propagated wave-fashion; and in others, that a change in the condition of the ponderable matter plays an important part. Thus, as I mentioned at the last meeting of the Association, I have found that in the discharge of a Leyden jar through a metallic wire, a series of rebounds between the inside and the outside of the jar takes place, precisely in the same way as the equilibrium would be restored by a series of waves, were a quantity of air, condensed in one vessel, suffered to discharge itself into another in which a vacuum previously existed. During this discharge, I have also shown that a series of inductions takes place, extending to a surprising distance on all sides of the wire; and as these are the results of currents in alternate directions, they must produce in surrounding space a series of plus and minus motions, analogous to, if not identical with undulations.

Next, that a change in the condition of the matter itself is required for the explanation of certain phenomena, will be evident from the following experiment: If portions of the same current of galvanism be sent through two parallel wires, or if portions of the same discharge from a Leyden jar be transmitted simultaneously through two parallel strips of platina foil, an attraction in both cases will be exhibited. If, however, the surface of a large circular metallic plate be covered at intervals with short needles placed parallel to each other, and a discharge of electricity be sent along the diameter of the circle at right angles to the needles, on examination, they will be found magnetized with different degrees of intensity: those in the direct line of the discharge will exhibit a slight degree of polarity, while those at the circumference of the plate will show a much greater amount of magnetic force; proving that the electrical discharge, instead of passing in the shortest line between the two points, has divided itself into two portions, each passing at as wide a distance as possible from the other. This phenomenon is in strict accordance with the hypothesis that the plate has been traversed by an elastic fluid, the particles of which, being self-repellent, have separated as far as possible from each other; and it can therefore be referred to the action of a fluid coexisting with, but independent of, ordinary matter; while the phenomenon of the attraction of the two parallel conductors before mentioned can only be explained by a change in the condition of the gross matter itself, combined perhaps with the action of an elastic fluid. I ought to state in this place that

my friend Dr. HARE, from purely theoretical considerations, independent of experiment, has arrived at a similar conclusion.

There is another phenomenon, which I may mention as producing a change in the properties of matter during the instantaneous passage of an electrical discharge. At the moment of the passage through the atmosphere of a discharge of electricity, the particles of the air are suddenly endowed with a surprisingly energetic repulsive tendency, to which is mainly to be attributed the mechanical effects produced by a discharge of lightning passing through a building. Also in the development of magnetism in a bar of iron or steel, a change takes place in the ponderable molecules of the metal : this is evident from the fact, that at the moment of magnetization, a wave of undulation, capable of producing an audible sound, is transmitted along the bar; and again, when the iron is demagnetized, if the expression may be allowed, a similar change in the position of the molecules is indicated.

In the explanation of the statical phenomena of electricity, we may either adopt the hypothesis of one or of two fluids, the mechanical results which are logically deduced from either being the same : in the case of the former, we have one movable and one fixed principle ; in that of the latter, we have two movable fluids and a fixed medium. It is evident that the mechanical results will be the same in the two theories, provided we suppose the absolute motion of the one fluid to be equivalent to the sum of the motions of the two fluids. Though either theory may be adopted with reference to the statical phenomena, the theory of one fluid is more readily applicable to the facts connected with electricity in motion, and particularly that part of the theory which assumes the activity of ordinary matter may hereafter be fruitful in new deductions.

The discoveries of the last few years have tended more and more to show the intimate connection of all the phenomena of the imponderables; and indeed we cannot avoid the conclusion, forced upon us by legitimate analogy, that they all result from the different actions of one all-pervading principle. Take, for illustration, the following example of the development of the several classes of phenomena. An iron rod, rapidly hammered, becomes red hot, or, in other words, emits heat and light. The same rod, insulated by a non-conductor and struck with another non-conductor, exhibits electrical attraction and repulsion. Again, if this rod be struck with a hammer while in a vertical position, it becomes magnetic. We have here the evolution of the four classes of phenomena by a simple

agitation of the atoms. We cannot, in accordance with the known simplicity of the operations of nature, for a moment imagine that these different results are to be referred to as many different and independent principles.

If we refer all these phenomena to one elastic medium, it will be necessary, in order to explain the facts of electricity and magnetism, that we suppose this medium to be capable of accumulation or condensation in certain portions of space; and of being lessened in quantity, or rarified, in other portions: also, that in its return to its normal condition, an actual transfer of the medium takes place. It follows from these assumptions, that the fluid withdrawn from one portion of space must leave an equivalent deficiency in another; or, in other words, that the amount of positive action must be equal in all cases to that of the negative. Further, since it appears from observation that the ethereal medium can only be condensed or accumulated in certain places by the insulating powers of ordinary matter, no electrical phenomena can be exhibited except in connection with such matter: hence electrical action can not be expected in the regions of celestial space.

The most difficult phenomena for which to invent a plausible mechanical explanation, connected with this subject, are those of the attraction of the two wires transmitting a current of electricity, and the transverse action of a galvanic wire on a magnetic needle. The theory of Ampère, though an admirable expression of a generalization of the phenomena of electro-magnetism, is wanting in that strict analogy with known mechanical actions which is desirable in a theory intended to explain phenomena of this kind.

In conclusion, I would again revert to the importance, in the adoption of mechanical hypotheses, of conditioning them in strict accordance with the operations of matter under the known laws of force and motion as exhibited in time and space.

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20. On Electrical Theory. By Dr. Robert Hare, of Philadelphia.

[*Not received.*]

IV. ASTRONOMY.

21. Observations on the Eclipse of the Sun, July 28, 1851. By Prof. Philip Ten Eyck, of Albany.

Made at the Albany Female Academy — Lat. 42° 39′ 12″; Lon. 73° 45′ 34″.

	h	m	s	
Beginning,	7	36	15,8	mean time.
End - -	9	19	15,9	"

Time. The beginning was observed with an independent seconds watch, which was compared with Mulford & Wendell's clock at 6^h 50^m and 7^h 50^m; the end, by my own pocket chronometer, compared with the same clock at 8^h 25^m and 9^h 30^m. The rate of the latter was nearly the same as the clock's. The former had a large rate, but, by frequent comparisons during the day, was found to be regular.

The error of the clock was determined by transits of six stars on the evening of the 26th, one on the 29th, and two on the 31st. The transit of Sirius over one line of the transit instrument, immediately after the eclipse (10^h 15^m), made a difference of $\frac{3}{10}$ of a second from the mean obtained from the other stars: but the heated atmosphere made the meridian mark appear so unsteady, that this was not used in the computation of time. The position of the transit instrument was observed before and after each transit, and it is not probable that the time is in error over half a second.

Telescope used. Reflecting Herschelian, made by Holcomb (Mass.): length 9 feet, diameter of mirror 8 inches clear, which was contracted to 4,8 inches to observe the eclipse. Single lens eye-piece; power 52 in diameter; dark glass pale green. My small Fraunhofer, erect eye-piece, power 28; dark glass pale purple.

Latitude of the Capitol, by observations made by James Ferguson in 1839, with Hassler's repeating reflecting circle....................					42°	39′	13,71″
By triangulation Academy south					0	0	1,84
Latitude of the Albany Female Academy..........					42	39	11,87
Longitude of the Capitol, according to Lieut. G. Thom, by chronometers, from Cambridge Observatory (Mass.)	4^h	55^m	$03,28^s$	or	73°	45′	49,2″
Diff.	0	0	01,01		0	0	15,3
Longitude of Albany Female Academy,	4	55	02,27	or	73	45	33,9

22. On the Solar Eclipse of July 28, 1851. By Lieut. C. H. Davis, U. S. N., Superintendent of the Nautical Almanac.

Lieut. Davis reminded the Association of a communication made by him at the meeting at New-Haven, concerning the solar eclipse of July 28 of this year; when he spoke of its value in determining the moon's semidiameter, and of the tables used at the office of the Nautical Almanac for determining the moon's place.

Those tables, it must be remembered, were new, and never before used for an ephemeris. They were intended by Airy, in their original form, as given in the volume of lunar reductions, to be an exact expression of Plana's theory, but had never been employed in practical applications. To these were added, under the immediate direction of Prof. Peirce, tables embodying Hansen's long inequalities, and Airy's corrections of the elements of the lunar orbit, and of some of the principal inequalities which he had derived from his discussion of the whole series of the Greenwich observations, together with some new terms to which this discussion had led him, the existence of which had never been before suspected, although Hansen's subsequent investigations have confirmed the results thus reached by an empirical process. It would have been very extraordinary if tables thus obtained had not proved superior to Burckhardt's old tables, which are still used in all the European ephemerides; and the observations of the present eclipse in this country have strikingly exemplified this superiority.

The chief difference in the results given by the two tables is in longitude : the other differences are less remarkable, and indeed of slight importance. The difference in longitude amounts to eighteen seconds of an arc in the case of the eclipse; and the difference in time for the phases of the eclipse was, for the beginning, rather more than a minute, and for the end, about two-thirds of a minute : and the whole of this great difference is in favor of the new tables, as is shown by all the observations without a single exception. To illustrate this point, I will cite the beginning of the eclipse for Cambridge, as follows :

	h	m	s
Computed by Paine	19	48	10
" " Amer. Naut. Almanac .		49	15,0
Observed		49	35,3

On the very day of the eclipse, I received from Mr. MIERS FISHER LONGSTRETH, of Philadelphia, a formula containing corrections of twelve other inequalities, with a statement that he had examined the remaining inequalities, and found that they required no correction. These corrections Mr. L. had derived also from Airy's lunar observations. The method by which these corrections were obtained, what they are, and their effect upon the theoretical computations, will, I hope, be communicated to the Association before it adjourns. I may say, however, that there was no doubt entertained, from the way in which they were computed, that they would be an improvement, though the admirable agreement that followed could never have been looked for.

Mr. DAVIS here introduced the observations, and compared them, making comments on the results, and concluded with expressions of the strongest admiration for the sagacity and invention which had led Mr. LONGSTRETH to his improvements of the formula of the longitude, by means of which we were now better able to predict the moon's place here than in any part of the world.

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23. ADDITIONAL NOTES OF A DISCUSSION OF TIDAL OBSERVATIONS MADE IN CONNECTION WITH THE COAST SURVEY AT CAT ISLAND, LOUISIANA. By Prof. A. D. BACHE, Superintendent U. S. Coast Survey.

IN my communication on the subject of the tides at Cat Island, coast of Louisiana, at the New-Haven meeting of the American Association, I showed that I had succeeded in decomposing the curves of rise and fall into a diurnal and semidiurnal curve, which were nearly curves of sines; the diurnal curve having its maximum approximately nine hours in advance of the first maximum of the semidiurnal curve, and the interference of these two waves producing the tidal wave as observed. The comparison of the curves deduced from the observations for three months, and the computed curves of sines, was shown to be satisfactory. This comparison, made as before by averages of periods of a week combined into one general mean, has now been extended to the whole year, as shown in the subjoined table. By increasing the maximum ordinate of the diurnal curve 0,02 of a

foot, which will make the rise and fall agree more nearly with the average deduced from observation, we obtain, as shown in No. 2, a resulting curve not differing in any ordinate more than a quarter of an inch from observation, and in which the positive and negative errors nearly balance, and the mean error deduced by summing the square of the errors is little more than one-eighth of an inch.

TABLE NO. I.

Showing the comparison of diurnal and semidiurnal curves deduced from observation, with curves of sines. DIAGRAM No. 1.

Hours from first mean level of water.	NO. 1. FROM OBSERVATION.			NO. 1. FROM CALCULATION.			Observation—calculation No. 1.	NO. 2. FROM CALCULATION.			Observation—computation No. 2.
	Diurnal curve.	Semidiurnal curve.	Mean tidal curve.	Diurnal curve.	Semidiurnal curve.	Mean tidal curve.		Maximum ordinate diurnal curve increased 0,08 foot.	Semidiurnal as before.	Resulting mean tidal curve.	
	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.
0	0,00	0,00	0,00	0,00	0,00	0,00	0,00	0,00	Same as foregoing.	0,00	0,00
1	,17	—,03	,14	,15	—,04	,11	,03	,15		,11	,02
2	,31	—,06	,25	,28	—,07	,21	,03	,29		,22	,02
3	,44	—,08	,36	,40	—,08	,32	,03	,42		,34	,02
4	,51	—,06	,45	,50	—,07	,43	,02	,51		,44	,00
5	,56	—,03	,53	,55	—,04	,51	,02	,57		,53	,00
6	,57	,00	,57	,57	,00	,57	,00	,59		,59	—,02
7	,56	+,03	,59	,55	+,04	,59	,00	,57		,61	—,02
8	,51	,06	,57	,50	,07	,57	,00	,51		,58	—,01
9	,44	,08	,52	,40	,08	,48	,03	,42		,50	,02
10	,31	,06	,37	,28	,07	,35	,01	,29		,56	,00
11	,17	,03	,20	,15	,04	,19	,01	,15		,19	,00
12	,00	,00	,00	,00	,00	,00	,00	,00		,00	,00

Nothing would be gained in closeness of representation of the result by displacing relatively the two tidal waves. It is only remarkable that in averages including the whole of the tides, even when most irregular, the results are so satisfactory. I have accordingly used the hypothesis of the representation of each wave by a curve of sines, deducing the maximum ordinate by computation from each observed ordinate. These laborious computations were made by ALEXANDER S. WADSWORTH junior, sub-assistant of the Coast Survey, and by Mr. P. B. HOOE. They gave tables of heights of the diurnal and semidiurnal curve for each day of observation, which form the basis of the discussion of the heights. The next step after decomposing the curve of observation into diurnal and semidiurnal curves, is to discuss each separately to ascertain if they follow the laws deduced from them in regard to heights and times.

I. DIURNAL WAVE : HEIGHTS AND TIMES.

If the diurnal curve is a curve of sines, then the ordinates found for each hour enable us to determine the value of the maximum or six-hour ordinate. Setting out from the mean line, then, we have for each day six determinations of the rise or fall above or below that line. Tables were computed from these, in which the daily curves were decomposed into their diurnal and semidiurnal components. In making these tables, the very irregular tides have been in general omitted. These tables were arranged according to the moon's declination, beginning and ending with the days on which the declination was zero, determining the maximum ordinate of each day from zero of declination. As the irregular tides occur near the time of the moon's passing the equator, the averages of the heights about these times are deduced from a less number of observations than the others, and are therefore less reliable. The following table gives the average heights, with the number of days from which they have been deduced. No advantage resulted from displacing the epoch of the moon's declination relatively to the day of highest tide.

TABLE NO. II. (DIAGRAM 2.)

Showing the value of the maximum ordinates of the diurnal curve, on the several days from zero of declination of the moon to zero again, with the number of days from which the results are deduced.

Days from zero of declination.	1	2	3	4	5	6	7	8	9	10	11	12	13	14
No. of observations.														
Heights	0,33	0,33	0,32	0,41	0,59	0,65	0,78	0,77	0,87	0,85	0,77	0,70	0,58	0,51
Nat. sin. 2 × moon's declination.	0,05	0,11	0,12	0,24	0,41	0,46	0,52	0,58	0,60	0,59	0,54	0,46	0,37	0,27

The dependence of the height of the diurnal wave upon the moon's declination appears by comparing the lowest line of the table, containing the sine of twice the moon's declination, with the line next above it : it is also shown by the curves of Diagram No. 2. This agrees with Mr. Whewell's approximate formula for the diurnal inequality, namely, $dh = C . \sin 2\delta'$; in which dh is the difference in height of two consecutive high or low waters, C a constant, and δ' the moon's declination.

The variation of this same height with the sun's declination may

be made at once apparent by classifying the heights for different values of the sun's declination with the same declination of the moon. The following table contains the greatest heights of the diurnal curves during the several lunations of the year, with the values of the sun's declination and of the moon's declination, grouped as described in the several columns.

TABLE NO. III. (DIAGRAM No. 3.)

Showing the effect of change of sun's declination on height.

Natural sine 2 sun's declination.	Number of lunations in group.	Natural sine 2 moon's declination.	Maximum ordinate diurnal curve.
Greater than 70°	5	,572	1,02
70 to 60	6	,577	0,99
60 to 40	6	,565	0,93
40 to 20	5	,530	0,94
20 to 00	4	,550	0,74

The effect of the change of parallax of the moon may be shown satisfactorily by grouping the values of the heights at the greatest southern declination of the moon, and for the greatest northern declination, for the year; comparing them for slightly varying declinations of the moon, for mean declinations of the sun, and for large variations of the parallax. The result is as shown in the following table, and in Diagram No. 4.

TABLE NO. IV.

Showing the effect of change of moon's parallax on height.

Number of results.	Mean sine 2 moon's declination both series.	Mean sine 2 sun's declination both series.	Mean parallax correct. for 1st series.	Mean parallax correct. for 2d series.	Mean height for lesser parallax.	Mean height for greater parallax.
13½	59,4	48,5	52,9	65,9	0,74	0,88

The parallax correction is taken as the cube of the parallax multiplied by the sine of twice the moon's declination.

These are the principal variable terms in the formula derived by Mr. Lubbock, from Bernouilli's theory of the tides, for the diurnal inequality, namely*,

$$dh = B[A.\sin 2\delta.\cos(\psi-\varphi) + \sin 2\delta'.\cos\psi];$$

in which dh is the difference in height of the morning and evening tide, B and A are constant coefficients, δ' is the moon's declination

* Transactions of the Royal Society of London, 1836, p. 223.

and δ the sun's; ψ is a small variable to be added to the mean luni-tidal interval to give the interval corresponding to the moon's age, and φ is the hour angle of the moon at the time of transit. The second term, introducing the parallax of the moon, would be

$$m.\frac{P'^3}{P^3}.\sin 2\delta'\,*;$$

in which m is a constant coefficient, P is the mean parallax, and P' the parallax at the time under consideration.

In the application of this formula to the observations, the maximum ordinates, found as before stated, were tabulated; and first the coefficients were deduced from the cases corresponding to the maximum of the sine of twice the moon's declination and to the minimum of the sun's, and *vice versa,* neglecting the small variations due to $\cos(\psi-\varphi)$ and $\cos\psi$. This gave the following values for the coefficients, and the two sets of equations derived conformed with each other.

TABLE No. V.

Showing the value of coefficients deduced from maximum sine twice moon's declination and minimum of sun's, and *vice versa;* neglecting variations due to $\cos(\psi-\varphi)$ and $\cos\psi$.

	$B.\cos\psi$.	$B.A.\cos(\psi-\varphi)$.
First six months...	1,07	0,43
Second six months.	1,00	0,39
Whole year.......	1,04	0,52

As each day's results are referred to the mean level of the day, and the mean of the low and high waters is taken as giving the height of the diurnal tide, the constant from the mean level of the whole should not appear in the values. In beginning these researches, I did not suppose that small differences would come out of them such as have been deduced. The reference to the level of each day compensated in a degree for the effect of an entire raising or depressing of the water by the winds' action.

The results promising success, the coefficients were deduced by the method of least squares for the first, and then for the second six months, and finally for the whole year. These laborious computations were made with much skill by Mr. W. W. GORDON, of the Coast Survey. The result for the second six months, in reference to the

* Lubbock's Elementary Treatise on the Tides, London, 1839.

coefficient of the term of the sun's declination, is discrepant from the final result; but as the coefficients for the whole year were used, after endeavoring to trace the error, if any, without immediate results, it was not pursued further.

TABLE NO. VI.

Coefficient of $\cos(\psi-\varphi)$, deduced from method of least squares.

	$B.\cos\psi$.	$B.A.\cos(\psi-\varphi)$.
First six months...	1,00	0,26
Second six months.	0,90	0,60
Whole year.......	0,96	0,24

The sum of the positive and negative quantities balance rather better by the use of the coefficients from the first method, which differs chiefly in the coefficient of the sun's action.

The coefficient of the first term of dh is $B \times (A)$, and of the second term B; and it will be seen hereafter in discussing the semidiurnal tide, that (A) is 0,364, which, with $B \times (A) = 0{,}26$, gives $B = 0{,}96$. The value 0,24 agrees, therefore, very well with that deduced by this different process.

A set of tables was next made, containing the values of the two terms of the formula for each day. To these was subsequently applied the small correction for the parallax from the term $\frac{P'^3}{P^3}$; and the terms, being summed, were compared with the observed maximum ordinate, and the difference in the final column of the table showed the residual to be accounted for.

For these tables I am indebted to Lieut. Trowbridge of the Corps of Engineers, assistant in the Coast Survey. The tabular quantities were also traced in curves, and then compared with the maximum ordinates. The positive and negative differences are usually small, not exceeding in the average about 0,12 of a foot, and are quite irregular.

The irregularities apparent in the phenomena themselves induced me, in first commencing this investigation, to hope merely to be able to trace the phenomenon generally; but it now appears, from the character of the results obtained from the averages, that the theory may be followed much more closely by the results than I had at first supposed.

The accordance of observation and theory, after the corrections

have been applied, is as good as the accidental errors of the separate results render necessary; as will be seen from the results for July given in the annexed table, and for July and part of August as given in Diagram No. 5: but as the averages seemed to indicate that the residuals would show the laws of the phenomena, I discussed them further.

TABLE NO. VII.

Showing the value of maximum ordinates of the diurnal curve, computed from the moon's declination and parallax, and from the sun's declination, compared with ordinates from observation, for the month of July.

(PART OF A TABLE FOR THE YEAR.)

DAYS.	Maximum ordinate.	$0{,}96 . \frac{P'^2}{P^2} . \sin 2\delta'$.	$0{,}26 . \sin 2\delta$.	
July 1	1,43	,66	,19	,60
2	,93	,59	,19	,17
3	,96	,35	,19	,43
4	,75	,33	,19	,26
5	,62	,22	,19	,17
6	,37	,08	,19	,10
7	,35	,14	,19	,12
8	,36	,14	,19	,03
9	,32	,25	,18	—,09
10	,52	,34	,18	,01
11	,65	,42	,18	,07
12	,75	,47	,18	,15
13	...	,54	,18	...
14	,78	,52	,18	,10
15	,73	,53	,18	,02
16	,62	,55	,18	—,10
17	,89	,53	,17	,19
18	,93	,48	,17	,29
19	,61	,40	,17	,06
20	,52	,15	,17	,20
21	,57	,00	,17	,40
22	,41	,02	,17	,22
23	,56	,33	,17	,07
24	,65	,45	,17	,05
25	,61	,55	,16	—,08
26	,77	,63	,16	,00
27	,90	,65	,16	,12
28	,86	,65	,16	,08
29	,90	,56	,16	,10
30	,90	,48	,16	,27
31	,69	,37	,15	,18

In looking for an explanation of the irregularities to the terms $(\psi - \varphi)$ and ψ, the residuals were classed according to the moon's

age, and the averages taken for the separate hours. The result of these tables is given in that annexed, which shows the residual for each six months and for the year. I have introduced them for the half year, to show that the same law is deducible, notwithstanding the irregularities of the individual results, from the observations for each six months.

TABLE No. VIII. (Diagram No. 6.)

Showing the residuals from the comparison of computed and observed ordinates of diurnal curves, classed according to the ages of the moon.

Hours of moon's transit.	Residuals.		
	First six months.	Second six months.	Mean.
0½	,23	,21	,22
1½	,17	,12	,13
2½	,15	,15	,15
3½	,15	,12	,13
4½	,16	,00	,08
5½	,08	—,03	,02
6½	,06	—,03	,01
7½	,08	—,02	,03
8½	,13	,04	,08
9½	,12	,12	,12
10½	,09	,14	,11
11½	,19	,14	,16

These residuals, instead of following the law of $\cos(\psi-\varphi)$, follow that of $\cos(2\psi-2\varphi)$, or that of the semidiurnal curve.

Before examining this result, which is shown in Diagram 6, I pass to the residual which is found by carrying on the former table to 23½ hours; which was in fact the form of the table before the development of the law of variation showed that the term for 12½ hours belonged to 0½, instead of 11½, with which it would agree if the law of $\cos(\psi-\varphi)$ were followed. The following table contains the residuals in question, shown also in Diagram No. 7.

TABLE NO. IX.

Showing residuals after deducting those following law of change of $\cos(2\psi - 2\varphi)$.

Age of moon.	Residuals.	Residuals.	Mean.
hours	feet.	hours.	feet.
$0\frac{1}{2}$	—,07	$23\frac{1}{2}$	—,01
$1\frac{1}{2}$	—,02	$22\frac{1}{2}$	—,01
$2\frac{1}{2}$	,01	$21\frac{1}{2}$	,01
$3\frac{1}{2}$	,03	$20\frac{1}{2}$	,03
$4\frac{1}{2}$	,00	$19\frac{1}{2}$	,02
$5\frac{1}{2}$	,01	$18\frac{1}{2}$	,04
$6\frac{1}{2}$	,05	$17\frac{1}{2}$	,08
$7\frac{1}{2}$	,04	$16\frac{1}{2}$	,09
$8\frac{1}{2}$	,07	$15\frac{1}{2}$	,04
$9\frac{1}{2}$	,02	$14\frac{1}{2}$	,00
$10\frac{1}{2}$	—,03	$13\frac{1}{2}$	,03
$11\frac{1}{2}$	,03	$12\frac{1}{2}$	,06
		Mean....	,03

The existence in the first residuals of the law belonging to the semidiurnal curve indicates that the separation of the two curves (diurnal and semidiurnal) is not complete, as indeed the hypothesis of a constant difference in time between the recurrence of the two maxima requires. Before undertaking to modify this hypothesis, I proceeded to inquire whether these numbers would receive modification from any other source. In examining the hypothesis that the component curves were curves of sines, a separation of the several hourly ordinates was necessary, and thus the four points at which the curves for twenty-four hours cross the line of mean level were brought into consideration each day. Two of these points varied necessarily considerably in position, while the two twenty-four hours apart were regular. Having found that the curves of sines represent very nearly the observations, the law thus obtained may be used in computing from all the hourly observations of the day the values of the maximum ordinates for each curve; forming the ordinates of the observed curve into groups containing respectively the same positive and negative values of the ordinates of the diurnal curve, and again of the semidiurnal, arranging the groups for the consecutive twenty-four hours. It was soon apparent that the ordinates for the semidiurnal curve would in this way prove more considerable, in the average, than in the former mode of computation, and that the results would be more regular; that the ordinates of the diurnal curve would, on the average, be slightly diminished, and in general prove

more regular. These revised tables have been prepared chiefly by Mr. W. W. Gordon and Mr. P. B. Hooe. They show on the average of the year a diminution of the maximum ordinates of the diurnal curve of 0,04 foot, and an increase of the maximum ordinates of the semidiurnal curve of 0,07 foot.

Classifying the corrections according to the moon's age, though they are irregular, it is apparent that there were entangled in the values of the former computed maximum ordinates, heights which belonged to the semidiurnal curve. The table of corrections for the two periods of six months, and for the year, is given below.

TABLE NO. X.

Showing the difference of maximum ordinates of diurnal curves, as computed by the last method of groups, and by that first applied.

Time of moon's transit.	Correction of maximum ordinates diurnal curve.		
	First 6 months.	Second 6 months.	Mean of year.
hours.	feet.	feet.	feet.
0½	—,10	—,06	—,08
1½	+,03	+,03	+,03
2½	—,08	—,02	—,05
3½	—,08	—,09	—,08
4½	—,08	—,09	—,08
5½	—,03	—,05	—,04
6½	—,02	—,02	—,02
7½	—,05	+,02	—,01
8½	+,01	+,02	+,01
9½	—,05	—,03	—,04
10½	—,08	—,03	—,05
11½	—,03	—,04	—,03

A consideration of the general formula for the height indicates a second correction. The height of high water, as given by the formula, is not the sum of the two greatest heights of the diurnal and semidiurnal tides. The hypothesis of the interference of the two waves makes the high water the sum of two ordinates (neither of which is the maximum), depending upon the laws of increase and decrease of the curves respectively, and of the relative position of the two ordinates. The correction due to this cause is readily found. The part of it which belongs to the diurnal curve will be the difference between D and $D.\cos(t-E)$; where E, according to the hypothesis of the interference of the two waves, is 9 hours; and t is the value for the maximum ordinate of the compound curve, namely (Proc. Amer. Assoc. Cambridge Meeting, page 289),

$$\operatorname{cosec} t - \sec t = \frac{4\,C}{D\sqrt{\tfrac{1}{2}}}.$$

This value of t, containing C (the maximum ordinate of the semi-diurnal curve), shows that the quantity will vary with the time of the moon's transit, according to the half-monthly inequality of the height. Following the course which I have taken throughout this communication to give the resulting tables merely, I subjoin the corrections thus derived from the tables for $\frac{4\,C}{D\sqrt{\frac{1}{2}}}$ from observation, the computed values of t, and of $D.\cos(t-E)$. The agreement of the general form of this correction with theory is a new confirmation of the values of the quantities C and D, deduced from observation, which it contains.

TABLE NO. XI.

Showing correction to height of the diurnal wave for difference of maximum ordinate, and of high water ordinate in compound curve.

Time of moon's transit.	Correction to maximum ordinate diurnal curve.
hours.	feet.
0½	—,08
1½	—,05
2½	—,03
3½	—,04
4½	—,04
5½	—,07
6½	—,08
7½	—,07
8½	—,06
9½	—,05
10½	—,05
11½	—,04

The correction furnished by the last two tables, and the corrected residual from the table, are given in Table No. 12 next following.

TABLE NO. XII.

Showing residuals after correcting for new computations of ordinates, and difference between high water and maximum ordinates.

Time of moon's transit.	Correction of residual.	Residual.	Corrected residual.
hours.	feet.	feet.	feet.
0½	—,11	,22	,11
1½	—,02	,13	,11
2½	—,08	,15	,07
3½	—,12	,13	,01
4½	—,12	,08	—,04
5½	—,11	,02	—,08
6½	—,10	,01	—,08
7½	—,08	,03	—,05
8½	—,05	,08	,03
9½	—,09	,12	,03
10½	—,10	,11	,01
11½	—,07	,16	,09
			+,21
		Mean....	,017

Comparing the residuals in this table with the uncorrected ones, we find their magnitude much decreased : the average is now less than 0,02 of a foot: but the form of the series is, as before, that belonging to the semidiurnal curve, and is as well marked as when the quantities were more considerable. Diagram No. 6 shows this fact; containing the curve of residuals from Tables 8 and 12, and of half-monthly inequality deduced from the observations. This persistence in the form of the residuals affords the best evidence that the irregularities of the observations, and changes in the mode of computation, do not introduce errors of sufficient magnitude to mask the laws of the phenomena. I propose therefore to modify the original hypothesis, so as if possible to obliterate this form in the residual.

Some collateral questions have been examined in the course of this discussion, the results of which are interesting. One of these is the comparison of the maximum ordinates of the diurnal curve, corresponding to the moon's declination north and south. The average value of the sine of twice the moon's declination, and the corresponding average maximum ordinate for northern and southern declinations, are shown in the next table; from which it appears that if the values of $\sin 2\delta'$ were equal, the heights would not differ appreciably.

TABLE NO. XIII.

Showing the mean value of twice the moon's declination, and the corresponding maximum ordinates for northern and southern declinations.

Sine $2\delta'$.	Maximum ordinate.	Sine $2\delta'$.	Maximum ordinate.
,410	,621	,351 ,410	,538 ,629

Another question was, whether the residuals, of which Table No. 7 shows a part, contained any portion which varied with the moon's declination. To test this, the residuals for six months were grouped according to the declinations, with the following result.

TABLE NO. XIV.

Containing the residuals after subtracting the terms containing the sine of twice the moon's declination, and the sine of twice the sun's declination, from the maximum ordinates, grouped according to the values of the sine of twice the moon's declination.

	Average value of twice sine moon's declination.				
Groups..........	0 to 20	20 to 35	35 to 45	45 to 55	55 to 70
Average value....	,151	,147	,169	,115	,167
No. of observations	(33)	(27)	(26)	(44)	(37)

The result indicates that there is no such term remaining in the residual.

Another question was, as to whether changing the epoch would improve the results. Several attempts of this kind were made at different stages of the work, but without any marked advantage. The average result for the year, as shown by comparing the dates of occurrence of the greatest and least maximum ordinate of the diurnal curve, and the greatest and least values of the term containing the moon's declination, is shown in the next table. The comparison is made in two different ways : first, by the date of the greatest value of the ordinate shown in the table of maximum ordinates ; and second, by the date shown by the highest point of the curve, which was traced to represent the observations.

TABLE NO. XV.

Showing results of comparison of dates of occurrence of the greatest and least maximum ordinate of the diurnal curve, and the greatest and least value of term containing the moon's declination.

DATE OF OCCURRENCE—AVERAGE IN DAYS.					
Maximum ordinate from table.	Maximum ordinate from curve.	Term embracing sun and moon's declination.	Minimum ordinate from table.	Minimum ordinate from curve.	Term containing sun and moon's declination.
15,4	16,1	16,0	16,5	16,6	16,0

The *times* of occurrence of the maximum of the diurnal curve are, as I have already stated, connected by the hypothesis with those of the semidiurnal curve. The times deducible from the observations were so irregular, that I supposed it impracticable to do more than this. Notwithstanding all these irregularities, it turns out that the laws of the phenomena for the times are deducible from the results. The average values follow those for the semidiurnal curve at the proper intervals. It will be practicable, therefore, to resume the examination of this part of the subject, which I accordingly purpose to do.

II. SEMIDIURNAL CURVE.

The results in relation to the semidiurnal curve have exceeded my anticipations. The half-monthly inequality, both in height and time, is very well shown by the maximum ordinates deduced; though the greatest value of the height is only 0,22 foot, and the irregularities in the separate observed high waters fall upon hours instead of minutes. In the following table, the maximum ordinates obtained by the method of groups are used, and the small correction for the difference between maximum and high water ordinates is omitted. The table contains the time of the moon's transit corresponding to the observed height; and the height computed from the formula given by Mr. Lubbock as resulting from Bernouilli's theory, and the difference between observation and theory.

TABLE NO. XVI.

Showing half-monthly inequality in height.

Hours of moon's transit.	Observed height.	Computed height.	O—C. Diff. of observed and computed.
0½	,220	,223	—,008
1½	,196	,206	—,016
2½	,199	,174	,025
3½	,147	,131	,016
4½	,132	,087	,045
5½	,074	,056	,018
6½	,047	,056	—,009
7½	,074	,087	—,013
8½	,113	,131	—,018
9½	,135	,174	—,039
10½	,133	,206	—,073
11½	,189	,223	—,034

The greatest difference between observed and computed heights is 0,073, and the least difference 0,003; and the mean, without regard to sign, is 0,026. Diagram No. 8 shows the observed and computed curves of half-monthly inequality of heights. The average interval corresponds to $2^h\ 35^m$ of the moon's transit; which is therefore the zero point, or epoch of the half-monthly inequality in the interval.

The interval corresponding to the moon's

	h	m		h	m
transit at	3	30	is	11	45
" for	9	30	"	13	05
			Diff. is	1	20

which, converted into arc, is 20°.

$$\text{Log tan } 20° = \log(\text{A}) = 9{,}56107:$$

$$(\text{A}) = 0{,}364; \qquad \frac{1}{\text{A}} = 2{,}747;$$

which is nearly the same as that obtained by Mr. Lubbock for Liverpool. The difference between the greatest and least heights is

$$(0{,}220-0{,}047) = 0{,}173.\text{E} = \frac{0{,}173}{2(\text{A})} = 0{,}238:$$

also the greatest height $0{,}220 = \text{D}+(\text{E})\times(1+\text{A}) = \text{D}+0{,}325$; and $\text{D} = -0{,}10$.

$$\text{Since } \frac{m'}{m'+\text{M}} = \frac{(0{,}07480)^2}{(\text{A})} = \frac{1}{65{,}06}, \quad \frac{m'}{\text{M}} = \frac{1}{64{,}06}.$$

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For the half-monthly inequality of the intervals, we have

$$\tang 2\psi = \frac{(A)\times \sin 2\varphi}{1+(A)\times \cos 2\varphi} = \frac{0,364\times \sin 2\varphi}{1+0,364\times \cos 2\varphi};$$

and in the heights,

$$\begin{aligned} h &= -0,10+(E)\times(A)\times\cos(2\psi-2\varphi)+(E)\cos 2\psi \\ &= -0,10+0,087\times\cos(2\psi-2\varphi)+0,238\times\cos 2\psi. \end{aligned}$$

The following table contains the half-monthly inequality of times deduced from the observations, and computed from the formula for tang 2ψ, and the comparison of observed and computed quantities.

TABLE NO. XVII.

Showing differences between the results obtained from the observations and from formula.

Mean from observation 12,35.

φ	ψ	C. From formula.	O. From observation.	O—C. +	O—C. —
h. m.	h. m.	h. m.	h. m.	m.	m.
0 30	0 08	12 27	12 31	04	
1 30	23	12 12	12 31	19	
2 30	36	11 59	11 19		40
3 30	42	11 53	11 45		08
4 30	38	11 57	12 03	06	
5 30	17	12 18	12 24	06	
6 30	17	12 52	12 38		14
7 30	38	13 13	13 09		04
8 30	42	13 17	13 27	10	
9 30	36	13 11	13 05		06
10 30	23	12 58	13 05	07	
11 30	08	12 43	13 05	22	
				+74	—72

$12^h\ 35^m$ not being the exact mean of the observed times, the + and — differences do not balance exactly.

Diagram No. 9 shows the observed and computed results. The greatest and least heights correspond with the average interval, as they should do by Bernouilli's theory. The mean lunitidal interval corresponds to $0^h\ 23^m$, which shows that a change of epoch of one transit would have made the mean interval agree with the transit at 0 hours.

24. On the Atmospheric Envelopes of Venus and other Planets. By Prof. Stephen Alexander, of Princeton.

Prof. Alexander made a brief allusion to the observations of Bianchini, Cassini and Schröter, on the spots and other phenomena presented by Venus; especially the extensive illumination beyond a hemisphere, which is most distinctly visible when the planet is near to its inferior conjunction. He mentioned various instances in which he had himself observed this latter phenomenon; and remarked, moreover, that the intensity of it was far too great to be attributed to a twilight produced by the planet's atmosphere, he having distinctly seen it when the planet was $1\frac{3}{4}°$ from the sun's limb. If, with Herr Mädler, it were attributed to an actual illumination of the surface by refraction, the extent of the refraction might be computed in the manner stated by Herr M. in the Astronomische Nachrichten. Prof. A., however, noticed an oversight of Herr Mädler with respect to one of the angles in question. Correcting this, the result of Herr Mädler's own observations would be about $1\frac{1}{3}$ times that which he had given in his paper in the Ast. Nachrichten already referred to. Prof. A.'s own results would, nevertheless, indicate a refraction nearly or quite the double of that obtained from Herr Mädler's observations. He remarked that this might very well be attributed in part to the irregular action of the planet's envelope, which sometimes seemed to vary greatly from one day to another; although the mean of his observations for 1839 would agree very well with that resulting from those of 1850 and 1851.

Prof. Alexander proceeded to remark upon the peculiar state of the envelope, which these irregularities seemed to indicate; and made other comments upon the state of the envelopes of Mercury, Mars, Jupiter and Saturn, and alluded to various phenomena presented by these and by the asteroids : from all which, he drew the conclusion that the envelopes of the asteroids approached to the nebular state; that the same was true, to an inferior degree, of Mars; that the envelopes of Mercury and Venus were still in a very unsettled state, and those of Jupiter and Saturn had very great density.

He recalled attention to a statement which he had made to the Association at its meeting in Philadelphia in 1848, having reference to the possibility of a mechanical action of the envelope of a planet upon some portion of the light which passes very obliquely through

it : that this might possibly have to do with the blue band observed in lunar eclipses, and certain phenomena of adhesion of the limbs in transits of Mercury and Venus; and concluded that the atmospheric envelopes of the planets might be quite unlike that of the earth, and that the effect of aberration was fully visible, because of our atmosphere's extreme porosity.

Prof. PEIRCE said, with regard to these atmospheres, he would mention the atmosphere pertaining to the inner ring of Saturn, discovered by Mr. BOND, and which he was inclined to believe was no ring at all. His analytical investigations were not yet completed; but he had almost come to the conclusion that that space between the outer and inner ring was but an irregular opening in the atmosphere of Saturn, which, from certain causes, might accumulate in such a position, that when the revolution of the planet had put the ring and atmosphere in certain positions, this vacancy in the atmosphere was perceptible to observers. There may be a deposition of vapor taking place near the edge of the ring of Saturn, which might have the appearance, and would account for the phenomenon as well, if not better, than by the proposition of Mr. Bond.

Prof. LOOMIS asked Prof. Peirce if this phenomenon had not been observed before, and commented on by astronomers of high rank?

Prof. MITCHEL had watched with much earnestness for an opportunity of observing these atmospheric phenomena; but he had been unfortunate, as he had never yet had an opportunity of seeing them. Perhaps the fault lay in the imperfection of the instrument which he used. Venus had particularly attracted him; and he had sought with much eagerness for a sight of the extraordinary phenomena which were sometimes exhibited on her surface, but he had never yet had that good fortune which had been accorded to other observers.

Prof. ALEXANDER stated that he had certainly seen spots on the disc of Venus, which had strongly attracted his attention. The instrument he used was a Fräunhöfer telescope of a three-inch aperture.

25. **Observations on the Zodiacal Light, with an Inquiry into its Nature and Constitution, and its relations to the Solar System.** By Prof. Denison Olmsted, of Yale College.

I submit to the Association a series of observations on the Zodiacal Light, made by me at Yale College, from 1833 to 1839, upon the basis of which I propose to offer a new description of this mysterious phenomenon, and a brief inquiry into its nature and constitution, and its relations to the solar system. Particularly, I propose to inquire whether or not it is the origin of the meteoric showers of November and August.

Various circumstances conspire to interrupt the continuity of a series of observations on the zodiacal light, among which are the following:

1. The comparatively few nights in the year when, in our climate, the sky is cloudless, and the atmosphere sufficiently clear to afford good observations on a light so feeble and diffuse.

2. The low angle which the zodiacal light makes with the horizon for the greater part of the year while it is visible.

3. The presence of the moon, which entirely effaces it; and, occasionally, for long periods, the presence of Venus or Jupiter, and sometimes of both planets. The light of Venus, especially, is often so bright, and the planet is so situated in the midst of the zodiacal light, as greatly to interfere with observations. Hence a number of years are necessary of diligent attention to the phenomena of this light, in order to become well acquainted with its habitudes and laws. Nor can I pretend to have made the best possible use of the opportunities afforded for viewing it, during the six years that my attention was directed to it. On the contrary, my observations were often interrupted by ill health, and other causes beyond my control. Still they were sufficient to convince me that my previous knowledge of this body was exceedingly defective, and my notions of it very erroneous; and the same may justly be said of most or all of the descriptions and graphic representations of it given in works of science.

I will, therefore, first attempt an accurate *description* and *representation* of the zodiacal light.

Since the direction of this body is oblique to the circles of diurnal revolution, and since it appears only immediately before or im-

mediately after the sun, and therefore more or less of it falls within the twilight, consequently its appearances are very different in different latitudes; being seen best of all in the tropical regions, where its direction always makes a high angle with the horizon, and where the twilight is short; and being scarcely visible in such high latitudes as London and Edinburgh, except near the time of the equinoxes. Hence British writers, who have attempted a description of it, have usually given one that is altogether vague and inaccurate. The lower latitude of our place of observation (41° 18′ 30″) affords a much better view of it, and my description and representation of it will conform to its appearance at this latitude.

I learn from my friend Prof. DANA, that while with the Exploring Expedition in the torrid zone, he seldom failed of seeing the zodiacal light morning or evening, when not prevented by some of the causes before enumerated; but during the summer months in our climate, we hardly see it at all. At the beginning of autumn we look for it in the morning sky, and at the end of autumn in the evening sky. The state of the atmosphere most favorable for seeing it at its minimum intensity, is that peculiarly transparent condition which either precedes or follows a copious rain. The presence of a black cloud, also, near the horizon, frequently enables us by contrast to see more distinctly the faint diffusive light of the upper portions. With these advantages, we may unite that of fixing one eye on a darker portion of the heavens a few degrees to the right or left, and looking askance with the other eye over the region of the object sought. This last expedient will usually be found useful for fixing its exact boundaries in its various stages of intensity.

Although, as was first remarked by Mr. E. C. Herrick, faint traces of the zodiacal light may be seen in the northeast early in August, yet it will hardly be obvious to common observation before the latter part of September. I quote from my record for September 25, 1835:

Observed the zodiacal light from 3 to 4½ o'clock, A. M. Very faint. Seen only by fixing the right eye on the region of Canis Major, and carrying the left eye along the ecliptic. Covers Regulus and the cluster in Cancer, and terminates a little south of Castor.

The earliest distinct view I have obtained of this body in the *evening* sky was on the twenty-first of November, 1837, when I have the following record:

Have constantly searched for the zodiacal light in the evening since the 13th

instant. Imagined that that part of the milky way where this light would cross it was more luminous than common, but the light is ambiguous on account of the presence of Venus; but this evening, examined it in company with three of my astronomical pupils, all distinguished for acuteness of vision. At 7 o'clock, Venus being near the horizon and hid behind a cloud, we could severally define the boundaries of the zodiacal light. By fixing the right eye on the milky way near Altair, and the left eye near the head of Capricornus, we could discern a pyramid less bright than the milky way, but still sufficiently distinct to be sure of its presence : its upper edge grazed Alpha and Mu Capricorni and Beta Aquarii, its vertex reaching to the right shoulder of Aquarius. Light very feeble and diffuse; but the triangular space between it and the milky way, embracing the Dolphin, perceptibly darker. Elongation from the sun 90°.

As a description of the zodiacal light, sufficient to guide the observer, I will offer the following. From the middle of September until the latter part of November, he will confine his attention to the *morning* sky. An hour and a half before daybreak (which is, at that season of the year in our climate, about four o'clock), he will first discern a feeble, diffuse, and scarcely visible light, of a pyramidal figure, extending from the horizon upward through the zodiac to Gemini, covering Regulus and Presepe, and terminating a little south of Castor. Near the horizon its material is usually mixed up with the vapors that prevail there, so as to prevent its forming a definite boundary at its base; but from an altitude of a few degrees above the horizon the light is at a maximum, whence it fades gradually upwards into nonentity. Along the central part of the pyramid, the illumination is greater than at the borders. From the greater length and amplitude revealed to us by circumstances peculiarly favorable for observation, we have reason to think that on ordinary occasions we do not see the whole of the body, but that it really extends further than its visible boundaries both in length and breadth. If the observer continues to watch this body from the middle of September onward through the month of October to the middle of November, he will perceive that the vertex or visible terminus moves along through the order of the signs, and nearly at the same rate with the sun; appearing, on the twenty-fifth of October, to occupy the space south of Denebola in the tail of the Lion, terminating a little above Regulus. From this time until the middle of November it appears nearly stationary, ascending from the horizon to the constellation Leo, in some part of which it terminates, the vertex varying somewhat in altitude with the condition of the sky. After the thirteenth of November, the light fades in the morning sky, contracts in dimensions, and soon becomes

stationary and then retrograde with respect to the sun; proceeding eastward no further than Gamma Virginis, a point which it reaches by the twenty-sixth of November, having at this time an elongation of only 60°, whereas a fortnight before the elongation was 90°. As the sun advances in the ecliptic, while the light appears nearly stationary, the elongation on this side continues to diminish, as well as the dimensions and the illumination, until early in January, after which it is scarcely seen in the east until August.

The foregoing general statements are supported by observations taken at different times through the period of six years before mentioned, a few of which I extract from my records:

Nov. 26, 1837. This morning about daybreak, saw the zodiacal light: very bright and distinct, but elongation only 60°.

Nov. 28. Commenced observations at five o'clock. Zodiacal light brighter than usual in preceding years at this season, but the vertex appears nearly stationary in Gamma Virginis.

Dec. 5. Zodiacal light visible this morning as early as three o'clock: not quite so bright as on the twenty-eighth of November, but increased in brightness from three o'clock till daybreak. Vertex still in Gamma Virginis.

Dec. 9. Examined the eastern sky from 4^h 30^m till daybreak: very cold and clear. Zodiacal light much less bright than on the fifth: width also less; when I first went out, could scarcely see it. Became distinct by five o'clock, half an hour before daybreak; yet much feebler than it was ten days ago. Contracted between Spica and Theta Virginis, 4° north of Spica; whereas a few days since the border grazed this star.

Jan. 18, 1837. Zodiacal light very diffusive and ill-defined. Seen after this no more in the east.

We will now introduce the observer to the *western* sky. Here the zodiacal light first comes into view, so as to be distinctly defined, about the twenty-first of November; at which time it lies far in the southwest, crosses the milky way, the head of Capricornus, and has its vertex near the right shoulder of Aquarius, with an elongation from the sun of full 90°. From this time it climbs rapidly upwards, until by December 2d it reaches nearly to Algenib in the equinoctial colure, having an elongation of more than 100°. By about Christmas the vertex reaches almost to Alpha Arietis, having an elongation of towards 120°. It becomes nearly stationary through the month of January; but in February and March it moves slowly onward through Taurus to Gemini, beyond which it scarcely advances. The accompanying diagram is intended to represent the general appearance of the zodiacal light, when seen under favorable circumstances near

the time of the vernal equinox. It is seen of a pyramidal form, with a broad base resting on the horizon. Its northern border grazes the bright star Algenib in Pegasus, passes south of Alpha Arietis seven or eight degrees, and about two degrees south of the Pleiades. Along its southern boundary we recognize the stars in the mouth and neck of the Whale, and, still higher, Aldebaran, the Hyades, and the horns of the Bull. The successive positions attributed to the zodiacal light from the time of its earliest appearance in the western sky, the twenty-first of November, to the vernal equinox, are not absolutely uniform; but they still correspond to observations made during the six years before mentioned, as will appear from a few extracts from my record book. I have already recited the observation of November 21st, 1837, when the return of this body to the western sky was first recognized.

Nov. 26, 1837. Light feeble, Venus being very bright; but seen after Venus was set, reaching nearly to the Fish south of Pegasus. Elongation 100°.

Dec. 2. New moon begins to interfere with observations; but this evening, the zodiacal light was visible after the moon was set: covers the pentagon in Pisces, and reaches beyond it. Elongation 110°.

Dec. 18. Early part of the day a violent rain and high wind: cleared off towards night. Zodiacal light very bright, reaching at least to Alpha Arietis; nearly as bright as the milky way. Elongation 120°.

It ought to be remarked that the phenomena of this body were peculiarly striking in the autumn and winter of 1837; and the observations made this year show a greater intensity of light, and a greater elongation from the sun, than those of corresponding dates in 1835 and 1836.

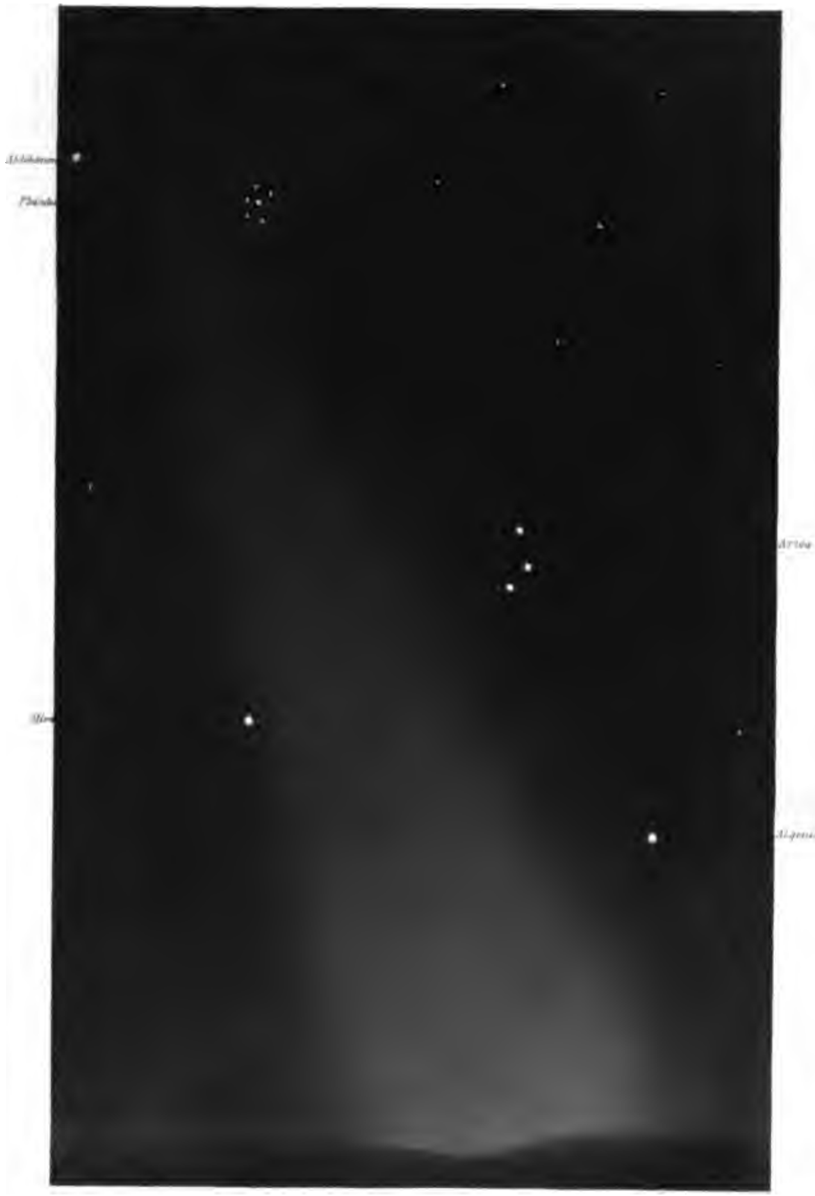
Dec. 21, 1835. This evening atmosphere very transparent. Zodiacal light very conspicuous, reaching nearly to Algenib, though quite faint towards the vertex. Elongation 90°.

Dec. 28, 1837. Night favorable. Appeared to me not to reach quite so far eastward as it did a few nights since; certainly not beyond the equinoctial colure. Could not be certain much further than the pentagon of stars in Pisces. Elongation 75°.

Feb. 7. Zodiacal light very conspicuous since the last moon, but has advanced eastward very little since Christmas, still reaching only to Alpha Arietis. Elongation 75°.

Feb. 24. First night since the moon has been away: sky favorable for observation. Zodiacal light bright and well defined, its axis nearly in the ecliptic; reaches to the space between Aldebaran and the Pleiades. Elongation 85°.

March 26. Zodiacal light very bright, reaching above the Pleiades, which are a little north of the axis. Elongation 60°.



ZODIACAL LIGHT

AT THE VERNAL EQUINOX LAT. 41°18'

March 29. Light more faint. Elongation 60° : vertex near the ecliptic.

April 6. Light fading rapidly : very diffuse.

May 1. Last night a very plentiful rain, after a series of warm days. To-day, air keen and sky very clear. This evening zodiacal light remarkably distinct (for this season of the year), being discernible much nearer the horizon than common, and reaching further eastward among the stars than I ever observed it before, namely, into the neighborhood of Castor and Pollux. Elongation 60°, but presumed to be much greater than it would be but for the extraordinary transparency of the atmosphere.

May 10, 1834. Zodiacal light seen for ten minutes after twilight ceased, say till ten minutes after nine : reached to Castor, but very diffuse. Elongation 57°. Seen no more in the west till the latter part of November.

To present at one view the various elongations from the sun, observed from November 21st to May 10th, the result is as follows :

1. November 21st,	elongation,	90°.	7. February 7th,	elongation,	75°.
2. November 26th,	"	100°.	8. March 29th,	"	60°.
3. December 2d,	"	110°.	9. April 6th, light rapidly fading.		
4. December 18th,	"	120°.	10. May 1st,	elongation,	60°.
5. December 21st,	"	90°.	11. May 10th,	"	57°.
6. December 28th,	"	75°.			

From this tabular view, it appears that when the body first came into view on the twenty-first of November, it extended about 90° eastward of the sun ; that its elongation increased rapidly from this period, being five days afterwards 100°, in six days more 110°, and in fourteen days after this 120°, which is the greatest elongation I have ever noticed ; and being at the same time about 60° westward of the sun, its whole extent in longitude was 180°.

I have, in a few instances, remarked what was apparently a sudden and remarkable expansion of the zodiacal light, a circumstance more than once noticed by Cassini. My record for November 21st, 1838, is as follows :

At 5 A. M., about twenty minutes before twilight, the zodiacal light was very large, extending in breadth from Corvus to Arcturus. Never saw it so broad before. More inclined towards the south than usual, its vertex passing one or two degrees to the south of Regulus.

Whether this extraordinary enlargement in breadth, implying a space of more than 40°, was owing to a change in the body itself, or to some unusual atmospheric refraction, or the accidental presence of an aurora borealis, it is impossible for me to decide.

It is well known that the great French astronomer, Dominique

Cassini, was the first to direct the attention of astronomers towards the zodiacal light; and that he made numerous observations on it, extending from 1683 to 1688 inclusive, which are published in the eighth volume of the Memoirs of the French Academy, together with observations on the same phenomenon made at Geneva by a friend of his, M. Fatio. An elaborate digest of these records was made by Mairan in his celebrated Treatise on the Aurora Borealis, including also a few observations of his own and of several other philosophers. It is interesting to compare these ancient observations with such as we have been able to make at corresponding times of the year; and having made this comparison in numerous instances, I feel able to say that *the zodiacal light, in the main, is the same thing that it was in the days of Cassini and Mairan;* being subject to similar variations at different seasens of the year, and in different states of the atmosphere. I shall avail myself of such aid as I can obtain from this and every other source in the remaining parts of this essay.

NATURE AND CONSTITUTION OF THE ZODIACAL LIGHT.

1. *Length.* The extreme portions of this body sometimes extend beyond the earth's orbit. It is obvious that, at an elongation of 90°, it must reach a tangent drawn to the earth's orbit at the place of the spectator; and if it reaches beyond that tangent, as is sometimes the case, it must of course extend beyond the earth's path. According to one of our observations, on the eighteenth December 1837, its elongation was 120°.

The variable apparent elongation to which this phenomenon is subject is more or less influenced by three causes : the state of the atmosphere, the inclination of its line of direction to the horizon, and the length of the twilight. In order to eliminate the effect due to atmospheric changes, we require numerous series of observations continued through successive years, and, if possible, instituted at long intervals of time. The mean of such an assemblage of observations would exhibit results nearly free from the effects of accidental variations in the transparency of the atmosphere. Since the axis of the zodiacal light does not deviate far from the ecliptic, we may imagine it to be represented by a portion of that circle on the artificial globe; and we shall easily see that since its inclination to the horizon varies between twenty-five and seventy-two degrees, being twenty-five at the vernal equinox (twenty-five degrees with the eas-

tern and seventy-two degrees with the western horizon), this cause must greatly affect the degree of intensity of the zodiacal light. The same must obviously be the case with the variations in the length of twilight; being an hour and a half after sunset at the vernal equinox, and two hours and a quarter after sunset at the summer solstice. But were these causes, combined, the only or the chief reason why the apparent elongation of the zodiacal light from the sun is greater at one time than at another, then, since at the vernal equinox the elevation above the horizon is at its maximum and the duration of twilight at its minimum, the apparent elongation ought to be greatest of all; whereas it is then only 60°, while, from the twenty-first of November to the eighteenth of December 1837, we found it increase from 90° to 120°, and this at a season of the year when the elevation above the southern horizon is near its minimum, and the duration of twilight is longer than before. Nor is this an anomalous fact: the elongation has uniformly appeared greater in the west, during the months of December and January, than during March and April. Again, at the winter solstice, the elevation is much greater in the morning than in the evening; but the light is far more conspicuous in the west than in the east.

2. *Direction.* The general direction of the zodiacal light is, as its name imports, from the sun along the zodiac. Cassini and Mairan thought that its axis lay nearly or quite in the plane of the solar equator, making an angle with the ecliptic of seven and a quarter degrees; and, accordingly, that its nodes must be in the part of the ecliptic which the earth traverses in June and November. But Cassini himself remarked that the direction of the axis is not always the same: on several occasions the vertex appeared to him to veer to the northward of its previous direction; so that while it would at one time just *graze* Alpha Arietis on its northern border, shortly afterwards that star would be wholly within it. Before I had met with these statements in Cassini, I had several times remarked the same changes in the direction of the axis, the vertex sometimes lying in the ecliptic itself: nor, as I think, will the observations warrant the conclusion that the axis of this body cuts the sun, and consequently lies across the ecliptic in the plane of a great circle. On the nineteenth of January 1835, the northern border was 8° south of Castor, and the vertex directed to a point south of the Pleiades: consequently its axis could not have been far from the ecliptic; but on the twentieth of March the vertex reached above the Pleiades,

and the axis had perceptibly veered northward from the ecliptic. These observations, taken in connection with those of Cassini, indicate that the supposed relation of this body to the solar equator is not constant. In the year 1843, M. Houzeau published an article in the Astronomische Nachrichten, in which he investigated the plane of symmetry of the zodiacal light from data derived from a comparison and digest of all the observations he could collect. He makes the inclination less than half that of the solar equator, and the place of the nodes of course quite different from that assigned to them by Cassini. If, then, as is demonstrated by Houzeau, the normal place of the axis gives it an inclination of only about 3½°, the great occasional deviations from this direction confirm our remark, that the course of the zodiacal light along the zodiac is not always the same, but is subject to vary with the seasons of the year.

3. *Motions.* The zodiacal light sometimes moves forward in the order of the signs; it is sometimes stationary among the stars, and sometimes retrograde. Beginning with morning observations in August, and noting its positions from day to day, we see it first stretching across the middle of the constellation of the Twins*. The vertex moves slowly along through the constellations Gemini, Cancer and Leo; being, on the thirteenth of November, a little east of Gamma Leonis†, having in three months shifted its place eastward nearly three signs, and consequently nearly kept pace with the sun in its annual revolution, maintaining an average elongation from that body of 90 degrees. After the middle of November its light fades away in the east, its vertex becomes nearly stationary, and of course its elongation westward of the sun diminishes, until the early part of January, when it is hardly visible at all in the morning sky. In the mean time, this light has been rapidly rising in the evening sky, and to this we will next direct our attention.

We have seen that about the twenty-fifth of November its upper portions reach beyond Capricornus, its vertex extending to the right arm of Aquarius. From this time it moves onward, sometimes more rapidly than the sun, but with an average elongation of 90°, until about the twenty-fourth of February, when it reaches a point a little south of the Pleiades. From the latter part of February, its progress eastward has seemed to me slower than before, hardly gaining one

* Above this point, the light is blended with that of the milky way.

† Cassini placed it in 1686 at Chi Leonis.

sign for the next three months, scarcely ever being distinctly visible beyond Castor; although neither the want of elevation above the southern horizon, nor the length of the twilight, would prevent its being seen beyond this, if in reality it existed there. Finally, early in April it rapidly fades away, and soon after the first of May disappears altogether.

These facts respecting the zodiacal light are derived chiefly from my own observations, made and recorded at different times during the six years following 1833; but on comparing them with the observations of Cassini made towards 170 years ago, a near correspondence will be found between them; and the same will be the case if the comparison be made with the tabular view of observations collected from various authorities, as given by Houzeau in 1843.

In some cases, the apparent progress of this body through the signs corresponds so nearly to that of the sun, as to suggest the idea that it is something attached to the sun, and has an apparent motion due to the same cause, namely, the motion of the earth in its orbit. In other cases, however, its movements are too sudden and too unlike those of the sun to permit such a conclusion. At one time, as we have seen, its elongation from the sun increases rapidly from 90° to 120°; at other times it becomes for considerable periods stationary among the stars, and even retrograde: facts which seem to imply motions of its own, independent of the sun and the earth; and such motions in any body thus situated, though they might be greatly modified by perspective, can hardly be any other than motions of revolution. On this subject Laplace has the following remarks, at the end of his chapter "on the figure of the atmosphere of the sun."

(1). "This atmosphere can extend no further than to the orbit of a planet, whose periodical revolution is performed in the same time as the sun's rotary motion about its axis, or in twenty-five days and a half: therefore it does not extend so far as the orbits of Mercury and Venus, and we know that the zodiacal light extends much beyond them.

(2). "The ratio of the polar to the equatorial diameter of the solar atmosphere cannot be less than $\frac{2}{3}$; and the zodiacal light appears under the form of a very flat lens, the apex of which is in the plane of the solar equator: therefore the fluid which reflects to us the zodiacal light is not the atmosphere of the sun; and since it surrounds that body, it must revolve about it according to the same laws

as the planets. Perhaps this is the reason why its resistance to their motions is insensible."

4. *Material.* The matter of which the zodiacal light is composed, presents many analogies to that of comets. In its visible form, in its direction with respect to the sun, in its very shade and color, in its increasing density towards the sun, in its transparency which, as in comets, is such as to permit small stars to be seen through almost every part of it; in all these respects, we recognize a great resemblance between the zodiacal light and the tails of comets. We are at least authorised to say that it is a "nebulous body."

From all the foregoing considerations on the nature and constitution of the zodiacal light, we infer, then, that it is a nebulous body, revolving around the sun in an orbit but slightly inclined to the ecliptic.

I proposed finally to inquire *whether or not the zodiacal light is the origin of the meteoric showers of November and August, and especially those of November.*

It may be known to some present, that after the great meteoric shower of November 13th, 1833, I published in the American Journal of Science some observations on the phenomena and causes of that remarkable exhibition of shooting stars; in which I came to the conclusion that they proceeded from a nebulous body revolving about the sun, and, at its aphelion, approaching very near to that part of the earth's orbit through which the earth passes on the thirteenth of November. At the conclusion of the essay, I suggested the possibility that the zodiacal light might be the body in question. I was reluctant, however, to insist on such a connection; because the existence of the nebulous body was inferred from evidence wholly independent of the zodiacal light, and even before the zodiacal light was thought of. In fact at that time I had very vague ideas respecting this light, as something that appears in the west after twilight about the time of the vernal equinox, but I did not even know that it was ever visible at the period of the year when the November meteors occurred; for at that time I had never read either the observations of Cassini on this body, or the treatise of Mairan on the aurora borealis, where so much is ascribed to its agency in the production of this latter phenomenon. Nearly twenty years have since elapsed, and I have had sufficient opportunity to observe the zodiacal light, and to reflect on the question of its possible connection with the meteoric showers of November and August: the result is an in-

creasing conviction of such a connection. I may here remark that the first idea of such an origin of the November meteors is now generally ascribed by European writers to M. Biot. It may be proper, however, to state that the paper in which M. Biot first mentions the subject, is an essay read before the French Academy soon after the meteoric shower of November 1836, three years after my paper was published. M. Biot does indeed favor the idea that these showers of meteors have their origin in the zodiacal light; but in noticing the the views which I had published respecting the cause of the meteoric shower, which he did me the honor to review at some length and in a manner very encouraging to myself, he distinctly stated that I had in my paper suggested the idea that the zodiacal light might possibly be the very nebulous body in question.

I am aware that the opinions I have formed differ widely from those entertained by many members of this Association, whose eminent talents and great success in the investigation of truth entitle them to the highest deference; but should I fail of convincing them of the correctness of my views, I still indulge the hope that I may secure increased attention to a natural phenomenon, which appears to me to have important relations to our solar system, as well as to several of the most sublime and mysterious phenomena of nature.

In the paper which I published in the American Journal of Science in the year 1834, on the cause of the great meteoric shower of November 13th, 1833, I inferred the existence in the planetary spaces of a nebulous body revolving around the sun, the extreme portions of which on the thirteenth of November lay over or across the earth's orbit, in such a manner that the earth passed through it, or at least near enough to it to attract portions of it into its atmosphere, where they took fire, and exhibited the phenomena of shooting stars. As the leading steps by which I arrived at this conclusion, after an extensive induction of facts, were very brief and simple, I may be permitted to repeat them here. I argued thus : If all the meteors which fell on this occasion (which were in vast numbers, and some of them proved to be bodies of comparatively large size), had been restored to their original position in space, they would of themselves have composed a nebulous body of considerable extent; but since the same shower had been several times repeated without any apparent exhaustion of the nebulous body, it was inferred that only small portions of that body came down to us, such as constituted its extreme parts which approximated nearest to the earth; and various

reasons induced the belief that the nebulous body itself was one of very great extent. It was a striking fact that the earth had, during several preceding years, fallen in with this body at exactly the same part of its orbit. Now since it is impossible to suppose that a body thus situated, and consequently subject to the sun's attraction, could have remained at rest in that part of the earth's orbit while the earth was making its revolution around the sun, the conclusion was that the nebulous body itself has a revolution around the sun, and a period of its own. Since the earth and the body met for several successive years at the same point of the ecliptic, that period must obviously be either a year or less than a year. It could not be more than a year; for, in that case, the body would not have completed its revolution, so as to meet the earth at the same point for successive years. Its period might be a year; and it might be less than a year, provided the time was some aliquot part of a year, so as to make it revolve just twice or three times, etc. while the earth revolves once. The time being given, we easily find the major axis of the orbit by Kepler's third law. On trying so short a period as one-third of a year, it gives a major axis too short to reach from the sun to the earth; and hence it was inferred that the body could not have so short a period as four months, since it would never in that case reach the earth's orbit even at its aphelion. A period of six months was found to be sufficient; and this was accordingly assumed at first to be the time, although the possibility that the period might be a year was distinctly admitted. But, extensive as I even then believed the nebulous body to be, I had formed very inadequate notions of its real extent; for this may clearly be sufficient to reach from the sun to the earth, and thus to correspond in dimensions to the zodiacal light; and since the centre of gravity of this body may be far within the earth's orbit, so its orbit may, even at its aphelion, be distant from the earth, and yet the extreme portions of the body may reach beyond the ecliptic. It would, therefore, be entirely consistent with my original views, to assign to a nebulous body of such an extent as that of the zodiacal light a period as short as one-third of a year, or even less.

I do not assert positively that the zodiacal light is the veritable body which produces the meteoric showers of November and August. Before such a hypothesis can be proved to be true or false, with certainty, a greater number of precise observations, continued through a series of years, would require to be made, and a careful

comparison instituted between the hypothesis and the facts. Should the zodiacal light be found at last incompetent to explain the periodical meteors, the existence of a nebulous body, as inferred from a full survey of the facts in the case of the meteoric shower of November 13th, 1833, independently of all hypothesis, will still be true. But, with great deference, I submit to the Association the following *presumptions* in favor of the opinion that the zodiacal light is the nebulous body which produces the meteoric showers of November:

1. The zodiacal light, as we have found in our inquiry into its nature and constitution, is a *nebulous body.*

2. It has a revolution around the sun.

3. It reaches beyond and *lies over the earth's orbit* at the time of the November meteors, and makes but a small angle with the ecliptic.

4. Like the "nebulous body," its periodic time is *commensurable* with that of the earth, so as to perform a certain whole number of revolutions while the earth performs one, and thus to complete the cycle in one year, at the end of which the zodiacal light and the earth return to the same relative position in space. This necessarily follows from the fact that at the same season of the year it occupies the same position one year with another, and the same now as when Cassini made his observations nearly one hundred and seventy years ago*.

5. In the meteoric showers of November, *the meteors are actually seen to come from the extreme portions of the zodiacal light*, or rather a little beyond the visible portions; and the same was true of the radiant point of the meteors (when watched, as it was by Mr. Fitch†, from October 16th to November 13th, 1837), namely, that the radiant always keeps the same relative position with respect to the vertex of the zodiacal light; being with that vertex in Gemini in the month of October, and travelling along with it through the constellation Cancer, and into Leo, where it was on the morning of the meteoric shower. Observations, so far as they have been made, indicate a similar relation between the meteors of August and the extreme portions of the zodiacal light.

These five propositions I offer as so many *facts* established by observation. Most of them appear in the original paper of Cassini

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* For the first suggestion of this analogy, I am indebted to one of my former pupils, Mr. Hubert Newton.

† Amer. Jour. Science, xxxiii, 386.

on the zodiacal light : others may be seen in the tabular collection by Houzeau of all the known observations made at different periods; a few, not noted by others, have been added by myself. For the inferences here made respecting the connexion of this body with the periodical meteors, I alone am responsible.

POSTSCRIPT.

Early in September 1851, accompanied by a few students of the senior class in Yale College, I commenced observations on the zodiacal light, and continued them on favorable mornings through the autumnal months. Having before me the original memoir of Cassini, I have felt interested in comparing his account of the same phenomenon at corresponding dates; being careful not to institute the comparison until after the record of each of my own observations was completed. The general agreement, in regard to position among the stars and other particulars, strengthens the conclusions I had before drawn, that *the zodiacal light is one and the same thing now that it was in the days of Cassini, one hundred and seventy years ago, and that its successive positions are the same at corresponding seasons of the year.*

September 3. Observed the zodiacal light from 3h to 3h 30m A. M. Sky very clear. Light feeble and diffuse. Lies between Pollux and Beta Canis Minoris. Vertex near the planet Mars, south of Epsilon Geminorum. Elongation 63°.

[CASSINI: *September* 4, 1685. La lumière paroissoit sur la poitrine des Jumeaux.]

September 25. From 4h till daybreak. Sky remarkably clear. Light very bright for the time of year. Vertex between the planet Mars (AR. 108°, Dec. N. 23°) and Pollux. North lat. 3°. Elongation 70°. North boundary line passes through Denebola and Epsilon Leonis. South line 6° south of Regulus. Breadth across Leo 17°. At the horizon exceeds 20°, but ill defined. Regulus and Presepe in the denser part, but both distinctly visible.

[CASSINI : *September* 19, 1687. A 4 heures du matin la lumière s'étendoit sur le Lion et sur l'Ecrevisse, et se terminoit à l'étoile de la poitrine des Jumeaux. Le cœur du Lion étoit pres-qu'au milieu de sa largeur : son côté septentrional passoit par les étoiles du col du Lion, et le méridional près de la tête de l'Hydre. *Sept.* 24. A 4 heures la lune se coucha; et après qu'elle fut entirement couchée, la lumière paroissoit plus clair sur le Lion, quoique la voye de lait à la même hauteur ne parût presque point. Sa longeur se terminoit insensiblement aux genoux des Jumeaux, de sorte que depuis le soleil elle étoit de 80 degrés.]

October 3. Sky perfectly clear. Some of the young men present could trace the light as high as Chi Geminorum, elongation 80°; but I was unable to see it above the Beehive.

October 7. Morning clear. Observed from 3h 45m, accompanied by thirteen young gentlemen of the senior class. Apex scarcely visible much above the Beehive: elongation 75°. During half an hour we counted 24 *shooting stars*, of which 19 came from the region around the apex of the zodiacal light, although the point of radiation was less definite than at the anniversaries of August and November.

October 8. Clear. At 4h 20m apex a little north of the Beehive, elongation 71°. Southern boundary veers a little to the north, coming within 5° of Regulus. *Shooting stars* again unusually frequent, but, with one or two exceptions, very small: counted 24 in half an hour, of which 20 came from the region of Cancer, but from an indefinite space of 10° around the Beehive. The following description would apply very nearly to the present appearances of the zodiacal light:

[CASSINI: *October* 8, 1687. Le 8 Octobre à 3 heures du matin la lumière parût fort claire sur la constellation du Lion, dont le cœur la divisoit inégalement; de sorte qu'un tiers étoit du côté du Midi, et les deux autres tiers du côté du Septentrion. Les pieds du Lion étoient à son terme méridional, et la moyenne du col à son septentrional; ainsi sa largeur étoit de 14 degrés. Elle ne passoit pas au-dessus du cœur du Lion.]

October 23. First morning without the presence of the moon. Not very clear. Observed from 4h to 4h 30m. Light seems to have veered still farther northward: scarcely visible south of Regulus; whereas, on the 8th, it was 5° south; but this in part at least is owing to its narrowing towards the vertex, which is approaching Regulus.

November 1. From 4h 20m to 4h 55m. Light between Regulus and Gamma Leonis. Elongation 85°.

November 6. From 4h 30m to 5h. Light well exposed, but less bright than sometimes heretofore. Elongation 85°. Apex 2° or 3° north of the ecliptic.

Shooting stars less and less frequent since the 8th of October.

Novemcer 13 & 14. Sky overcast.

November 20. First morning could observe, on account of the moon. Observed from 4h to 4h 30m. Light very indistinct and diffuse. Vertex not higher than Eta Leonis; scarcely perceptible above Chi Leonis. Saw it last evening immediately after twilight, extending towards the head of Capricorn, but very faint and diffuse.

December 2. Cold and clear. Light grazes Spica Virginis, on its southern border. Apex near Gamma Virginis. Elongation 63°.

[CASSINI: *December* 4, 1685. Le 4 Decembre à 5 heures 15 minutes du matin la lumière s'étendoit sur la partie inferieure de la Vierge, et se terminoit insensiblement près de la ceinture à 68° de distance du soleil. Elle comprenoit les autres étoiles de la Vierge au-dessous de la ceinture jusqu'aux pieds, et celles que l'on voyoit de la Balance.]

December 16. At 6h 30m P. M. Ill-defined: northern boundary passes through Alpha Capricorni and Beta Aquarii. Vertex diffuse and faint, among the stars of the pentagon in Pisces. Elongation eastward of the sun, 85°. Cassini, Dec. 18th, 1685, gives the elongation 86°.

26. On the Origin of the Forms and present State of some of the Clusters of Stars, and resolvable Nebulæ. By Prof. Stephen Alexander, of Princeton.

Every explanation of a physical phenomenon should be adequate in two respects : in mode, and in measure. This may be illustrated by an example or two.

Whiston attributed the deluge to the near approach of a comet. The explanation was appropriate in mode, since the attractive force of such a body would tend to raise the waters of the ocean, *i. e.* cause a tide; but the explanation was inadequate in measure, as the mass of the comet would be insufficient, and the comet could be near to the earth for but a very short time.

An explanation of a celestial phenomenon which should suppose the rotation of the heavens to be real, would, in many cases, be right in measure; since the time, and some other circumstances, would not be altered in measure, if such a hypothesis were tenable; but every explanation founded upon it would be wrong in mode, since the very reverse of such a hypothesis is indeed the fact.

The nebular hypothesis was admitted long since to be right in mode : the happy application by Mr. Walker of Prof. Kirkwood's beautiful analogy, has shown it to be very probably right also in measure.

The considerations which I have presented in the Astronomical Journal of March last, go to show that the asteroids and comets of short period have a common origin; or that masses of matter which are now confessedly, some nebulous and some planetary, have been once associated.

The communication which I last year made to the Association at New-Haven, tended to show that the sun was anciently a body surrounded by two rings similar to those of Saturn; and that from the inner ring were formed the planets from Mercury to Saturn inclusive, while from the outer ring were formed Uranus and Neptune, if no more. If the liquid state of the rings, which Mr. Bond and Prof. Peirce have shown to prevail in the case of Saturn, were also admissible in the case of the sun, then the rending of those rings might give rise to the planets now known to exist, and their relative size would be in some measure accounted for.

This allusion to the nebular hypothesis and its applications is not unimportant, since it has in some measure furnished the basis for a much more extensive generalization.

At least two forms of equilibrium of a given rotating mass are usually admissible; both spheroids, but the one of a vastly greater ellipticity than the other. In the case of the earth supposed homogeneous, one of these forms would approach very nearly to that which at present exists; the other would exhibit a ratio of the axes of 680½ to 1.

The material of which some of the clusters and resolvable nebulæ were formed, may have been,

1. A fluid spheroid of great ellipticity; the gradual cooling of which might increase its velocity, and produce a rupture and dispersion which would respectively give rise to the present forms of the spiral nebulæ observed by Lord Rosse. The milky way may have this form.

2. A ring may have been the primary form, or a spheroid may have been transformed into a ring, the subsequent rupture of which might give rise to other recognized forms.

3. The simultaneous rupture of a ring might give rise to the annular nebula in Lyra and others.

4. The simultaneous rupture of a spheroid might give rise to the "Dumb-bell" nebula and others.

5. Globular nebulæ also show traces of similar action.

These changes are still, as it would seem, progressing; though their progress can scarcely be sensible for a century or more, because of the vast scale on which the changes must take place.

Prof. Mitchel desired to express the intense interest with which he had listened to the paper of Prof. Alexander; and also his surprise, since when he himself was about to commence a similar investigation some years ago, he found upon his table a note begging him to desist from such an "*atheistical*" attempt! That day, however, he trusted, had gone by. He added, that his own observations confirmed the views of Prof. Alexander.

27. Statement of the Results of a set of Observations in repetition of the Foucault Experiment. By Professors Caswell and Norton.

The substance of these observations was communicated by Prof. Norton.

The pendulum was suspended in a tower at the Railroad Depot building at Providence, and was 97 feet in length; the weight was a little less than 40 lbs. It was a ball of lead, nearly spherical; through the centre of which passed a perfectly straight steel rod, about $\frac{1}{4}$ of an inch in diameter. This rod was tapered to a point below the ball. At the other extremity it was provided with a cap, to screw on and off, for the more convenient attachment of the wire, which passed through a small hole in the centre of the cap. The leaden ball was cast around the rod, and then turned in a lathe.

By careful trial, it was found that the centre of gravity was truly situated on the axis. The wire by which the weight was suspended was of brass, half annealed, and was 0,035 of an inch in diameter. It was attached at the top to a screw bolt, passing through a small hole in the head of it.

All the openings in the tower were carefully closed up, to secure as tranquil a state of the air within it as possible. Beneath the weight was placed a circular table, a little over five feet in diameter, the circumference of which was divided into degrees. The point of the rod, when the weight was at rest, was directly over the centre of the table, and within a small fraction of an inch of it. The table was provided with an index, movable around its centre, and extending entirely across it and somewhat beyond the outer circular rim. At each end of this index was an upright sight-vane, having a slit traversed by a fine thread, and also a vernier for reading the angles. A black thread was stretched across from the zero of one vernier to that of the other, and passing through the centre of the table.

In starting the pendulum, the weight was first divested of any tendency to rotatory movement, and then carefully and slowly drawn to one side, and attached to one of the uprights by a loop with a single cotton thread leading from it. The point of the rod below the weight was then adjusted to the zero of the contiguous vernier; and after it had become perfectly still in this position, the thread was burnt. The arc of oscillation traversed by the weight was about

$5\frac{1}{2}$ feet, and the time of oscillation 5 seconds. The movement of the plane of oscillation was followed and measured by moving the index. The observations were generally made every ten minutes, and the angles read by both observers, each observer having charge of a vernier, and usually were continued for an hour : in one instance, they were continued for three hours. Whenever the elliptic movement of the weight, which was found almost always to arise after a time to a greater or less extent, became very marked, the period of observation was shortened to half an hour, or even less. The observations were made at all parts of the circle; that is, the pendulum was started from various points, so that the plane of oscillation might, at the outset, have every variety of position.

It will be perceived that the observations were conducted in a manner calculated to eliminate errors of graduation and eccentricity. It ought to be stated, however, that the number of observations hitherto made is not sufficient to warrant the conclusion that the mean of the results of the individual observations is very exact, especially as the graduation of the circle was made by hand. The number of observations of the movement of the plane of oscillation, during an interval of an hour or half-hour, is 23. In the case of four of the observations, the elliptic movement was so large that the results cannot be relied upon; and in the case of three others, the results differ so widely from the mean of the others, that they must be regarded as anomalous, and it is accordingly believed that they ought to be rejected. The mean is therefore derived from only 16 observations : it can only be regarded as an approximation to the result sought. It is proposed to continue the observations, if practicable, in the hope of obtaining a closer approximation.

According to the sixteen observations taken into account, the mean motion per hour is 9° 57,3′. The theoretical motion, in the latitude of Providence, is 10° 0,1′; that is, 2,8′ greater than the result of the observations. As to the amount of the elliptic movement observed, there was only one instance in which none was perceptible during an entire hour : in the other instances, the conjugate axis was between zero and half an inch in length. In the case of the observations rejected, it amounted to an inch and a half or more. The slowest motion observed was 8° 39′, and the most rapid 10° 54′.

It does not appear that the irregularities observed are attributable to the elliptic movement. Some of the largest deviations from the

general mean occurred when this movement was very slight. As to the direction of the elliptic revolution, it was, in the majority of instances, indirect, or contrary to that of the motion of the hands of a watch. In only four instances out of twenty-three, was a motion in the contrary direction observed.

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28. On the Pendulum Experiment. By Prof. J. D. Dana.

[*Not received.*]

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29. The Pendulum at Bunker-Hill Monument. By Professor E. N. Horsford.

The interest felt by the public generally in the great experiment of Foucault, and the attention which has been directed to the repetition of this experiment at the Bunker-Hill Monument, will justify a brief account of the apparatus there employed, and the results there arrived at.

The ball used for the pendulum is a thirty-two pound iron cannon ball. It was thrown out in some excavations on Charlestown Neck, some years since, and is supposed, from the number of others of the same size found in the same locality, to have been discharged from the British ship of war Lively, at the American army, near the close of the battle on the 17th of June 1775. The weight of the ball is now about thirty-one pounds. The ball is supported in a brass meridian, to which is attached an equatorial ring, with adjusting screws for bringing the centre of gravity directly in a line with the index below and the point of suspension above.

The ball is suspended by a wire 210 feet long (known as No. 20 steel wire), which is annealed, and secured to the brass meridian of the ball by a small screw clamp : the other end of the wire is secured in a similar clamp, which is attached to a staple in the roof of the chamber at the head of the Monument stairs. In making the arrangements, it became necessary to remove the grating in the floor of the chamber above, and also the marble monument erected by King

Solomon's Lodge within the well at the bottom. To prevent currents of air from affecting the motion of the pendulum, the grating above has been replaced by a plank floor; and the wire passing through the chamber is encased in a square wooden trunk, with panes of glass on opposite sides, for observing the very small arc described by the pendulum wire at this height. To avoid obstructing currents of air in the shaft, it has also been found necessary to close the ventilating port-holes opening into the inner shaft, and replace the iron lattice gate at the bottom with plate glass doors. A smooth wooden floor has been laid at the bottom of the shaft; from the centre of which, directly beneath the point of suspension, three circles have been described and painted white. These circles are graduated into 360 degrees, which are figured for the convenience of observation, counting from the right. For more careful observation, a flat wooden ring is erected about four feet above the floor, with a brass sight on the further side, and a corresponding sight at the extremity of an arm on the nearer side, which is so arranged as to revolve around the axis of the plane of oscillation. This ring rests upon marbles, and may be adjusted by movable friction wheels placed outside the ring, upon the inner wall of the monument. With the aid of these compass sights, the rotation of the plane of oscillation becomes apparent in less time than if the point at the bottom of the ball be observed; for the reason that the view is confined to a definite line, and also that the distance of the nearer sight from the centre is five feet, while the radius of the graduated circle on the floor is rather less than three and a half feet. The sight, through which the observation is made, is moved by a small geared wheel upon a graduated arc of brass placed in the doorway; and such is the nicety of the adjustment, that the progress of the plane of oscillation can be noticed in a single vibration of the pendulum.

The mode of starting the pendulum is that adopted by Foucault. The ball being drawn to the margin of the circle, and secured by a thread, is permitted to come entirely to rest: when this is attained, which requires but a minute or two, the thread is burned and the pendulum launched.

An unanticipated difficulty, in the movement of the monument itself, and to which more particular reference has been made in another communication, for some time interfered with the progress of experiment. The impossibility of using the graduation on the floor, led to the adoption of a movable section of a sector of brass, of three

feet radius, having the degree divisions prolonged six inches towards the centre, to prevent the lessening of the length of sweep from interfering with observation. The position of this plate with regard to the centre could be determined at any moment, by bringing the pendulum to rest, and measuring from the so ascertained centre.

Another difficulty met me at this point. The armature in which the ball was slung, notwithstanding the extreme slowness of the movement (an oscillation requiring less than $8\frac{1}{16}$ seconds) interfered seriously with the experiment.

Sometimes the plane of oscillation would sweep through a degree in four minutes, and sometimes it required eleven. A series of experiments eliminated the source of this difficulty, and pointed out the mode of so managing the apparatus as to secure trustworthy results. The vertical brass ring gave to the ball a slightly lenticular form; which, it is obvious, can oscillate uniformly in but two directions, to wit, in the direction of the axis of the lens, and at right angles to it. In either of these, I obtained correct results. As admitting the least tendency to error, I uniformly adopted the latter direction, that is, in which the plane of the ring coincided with the plane of oscillation. My observations were limited to a single degree, and that the first: this was necessary from the diminution in the length of the sweep, unless I had constructed a movable floor. It had this advantage, that the elliptical motion which, after a longer or shorter period, invariably sets in, was uniformly not appreciable. I have occasionally watched the motion of the pendulum when it started under the fairest circumstances, considering the imperfection of my ball, through a period of twelve minutes, without detecting elliptical motion, though frequently it came on in less time. Prof. Norton, of Brown University, has informed me that in experiments conducted by Prof. Caswell and himself, the elliptical motion was frequently not observable for half an hour. The elliptical motion was, with very rare exceptions, in the direction opposite to the hands of a watch. The greatest conjugate diameter of the ellipse described has rarely exceeded half an inch. This maximum, which frequently was gained in less than twenty minutes, continued for four hours, the ball itself not coming to rest in less than five or six hours. The length of the arc of oscillation was six feet, the whole diameter of the well being but seven feet. The time required for a single oscillation was $8\frac{1}{16}$ seconds as already mentioned, losing one second in 224 oscillations.

In observation, I at first had the aid of an assistant, who counted the oscillations, while I observed the time required for the rotation of the plane of oscillation through a single degree. It was possible to observe only at one extreme of the oscillation. The determination of the time by oscillations, or by seconds, if either one oscillation too many or too few, gave a range of error of $32\frac{1}{4}$ seconds. It so happened that the length of the pendulum at the Monument, and the theoretical time at that locality, were marvellously suited to each other. The time required (I take the calculation by Prof. Caswell for the State House, Boston) was 5^m $56{,}03^s$. Forty-four oscillations, at $8\frac{1}{16}$ seconds for an oscillation, give 5^m $55{,}5^s$.

In my observations, a second or two might be lost at the outset in carrying the eye from the burning thread to the watch face; and at the conclusion, in glancing from the index coincident with the degree division, to the second hand. But when not varying more than three or four seconds, it would be just to correct the time by the oscillations, since the time required for an oscillation had been ascertained with the greatest precision.

I will here give a few of the observations, with their average; and with it the time indicated by the oscillations, and that by theory*.

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* I should not omit to state, that the conveniences for these observations have been furnished by the Massachusetts Charitable Mechanics' Association, who provided the necessary funds for the pendulum apparatus. The committee appointed by them, consisting of Messrs. Wightman and Simpson of Boston, and Mr. Bond and myself of Cambridge, were facilitated in all their labors by the officers of the Bunker's Hill Monument Association.

N. 72° W. to N. 71° W.			S. 43° W. to 44°.		N. 43° W. to N. 42° W.	
By time.	By oscillation.	By theory.	By time.	By theory.	By time.	By theory.
5' 54"	5' 54",75	5' 56",03	5' 54"	5' 56",03	5' 54"	5' 56",03
5 54	...	...	6 04	...	5 50	...
5 49	...	...	5 55	...	5 56	...
5 53	...	...	5 56	...	5 54	...
5 56	...	...	5 55	...	5 55	...
5 57	...	...	5 55	...	5 54	...
5 57	...	...	5 54	...	5 55	...
5 50	...	...	5 54	...	5 54	...
5 60	...	...				
Average, 5' 55",55	5' 54",75	5' 56",03	Average, 5' 57",66	5' 56",03	Average, 5' 54"	5' 56",03

These results do not confirm the suggestion that there is greater or less than the uniform theoretical velocity of rotation of the plane of oscillation in different parts of its orbit.

Soon after the newspaper accounts of the experiment at the Pantheon reached this country, I tried an experiment suggested by a writer in the *London Illustrated News;* that of suspending a horizontal bar. It was the view of the writer that the bar would rotate, as the plane of oscillation rotates.

I suspended a copper bar, six inches long, in a glass receiver, by a slender iron wire terminating in a point, on the polished face of a straight magnet. By additions to the weight of the bar, I reduced the friction on the magnet to the least measure. The receiver stood upon a window sill in a brick building; and the bar having come to rest, it continued at rest through forty-eight hours.

At a later period, I suspended in the Monument, from a height of 190 feet, a glass tube two feet long, laden with lead at each end. The first 60 feet of suspension was of silk braid, to reduce the torsion to a minimum : then came iron wire to within a few feet of the bar, and then a brass wire. So sensitive was the bar, that I found it impossible, so long as I remained near it, to bring it to rest, the unavoidable currents of air from my breath being sufficient to prevent its becoming still. I, however, reduced its oscillations to a range of four or five inches at the extreme of the bar, and then left it over night : in the morning, it was entirely at rest. This was repeated under such favorable circumstances, and with such coincident results, as to induce me to believe that those experimenters who have noticed a bar, or disk, or wheel, to revolve in a time corresponding with the plane of oscillation, have been misled by some concealed source of error.

I repeated the experiment of permitting water to flow from a funnel, in the expectation that the resultant of the motion toward the centre in flowing out, united with the possible rotation of the body of water, would give rise to the spiral movement with the sun, which is very generally believed.

I took for this purpose a tub turned in a lathe, and carefully bored a half-inch hole in the centre of the bottom. Into this hole I passed a good cork from below. Having filled the tub (which contained about fifteen gallons), the water was permitted to come to rest, as indicated by particles of sawdust in the water and on its surface. When quiet was established, which required nearly an hour, the cork was withdrawn. The water flowed out with the following results in seven trials : In two, there was no whirl; in three, the whirl, just perceptible in the last quart of water, was with the sun; in the remaining two, the whirl was in the opposite direction.

These results seem to indicate, that in but two of the seven trials, had the water obtained actual rest; while it had, in three of the other instances, a slight motion in one direction, and in two a motion in the opposite.

30. Notes on the Tides at Sand Key, near Key West, Florida. By Prof. A. D. Bache, Superintendent U. S. Coast Survey.

Hourly observations of the tides at Key West, Florida, were commenced under instructions from, and the immediate direction of Lieut. John Rodgers, U. S. N., Assistant in the Coast Survey, in January last. The results are of great importance in regard to the general subject of the tides in the Gulf of Mexico. The observations up to June have been plotted in diagrams, and are in the course of reduction. To show their peculiar features, I have presented in a diagram cases of tides observed at or near the maximum of the moon's declination at syzigy and quadrature, and near the zero of declination and syzigies. It will be seen that there are two high and two low waters daily at Key West, the successive tides being nearly equal, at or near the time of the moon's passing the equator; and there being one small tide and one larger at other periods, making the diurnal inequality a very remarkable feature of the tide.

I find by examining these in the way done for the tides at Cat Island, Louisiana, that the results may be represented by the interference of a semidiurnal and diurnal tide wave; the latter being approximately nine or twenty-one hours in advance of the former in the position of its maximum ordinate, as has already been shown for the tides at the other locality.

I propose, at a future meeting, to present the discussion of these tides to the Association; and at present refer to the tables and diagrams showing the decomposition of the curve of observation, and the agreement of the curve computed from the elements with the curve of observations. The average difference in the computed and observed ordinates is less than four-hundredths of a foot.

A consideration of the mode in which the maximum or minimum ordinates of the tidal curve are derived, will show that the difference of the two high and low waters is made up of four unequal quantities; two of which give the difference between two (generally) unequal ordinates in different portions of the diurnal curve, and the other two the difference of two ordinates not symmetrically situated in the semidiurnal curve. If this be so, this difference should not follow the law of the diurnal wave.

I. MAXIMUM DECLINATION OF MOON.

SAND KEY, *January* 29, 1851.

Moon's declination S. 20° 46′; Moon's transit, 23^h 03^m.

(TABLE NO. I.—DIAGRAM No. 1.)

Hours.	Observed.		C. 0,60.	D. 0,60.	Computed.	O — C.
0	,00		,00	,00	,00	,00
1	—,12		—,30	,15	—,15	,03
2	—,26		—,52	,30	—,22	—,04
3	—,23		—,60	,42	—,18	—,05
4	,00		—,52	,52	±,00	,00
5	,33		—,30	,59	,29	,04
6	,63	0,35	,00	,60	,60	,03
7	,83		,30	,59	,89	,06
8	1,10		,52	,52	1,04	,06
9	1,08		,60	,42	1,02	,06
10	,80		,52	,30	,82	—,02
11	,38		,30	,15	,45	—,07
12	,00	4,19	,00	,00	,00	,00

$$
\begin{aligned}
-3{,}74\,\text{c} + 4{,}29\,\text{d} &= 0{,}35 \\
3{,}74\,\text{c} + 3{,}29\,\text{d} &= 4{,}19 \\
\hline
7{,}58\,\text{d} &= 4{,}54 \\
\text{d} &= 0{,}60 \quad \text{diurnal;}
\end{aligned}
$$

$\text{c} = 2{,}22 \div 3{,}74 = 0{,}60$ semidiurnal curve.

Hours.	—,07.		C. 0,51.	D. 0,42.	Computed.	O — C.
12	,00		,00	,00	,00	,00
13	—,40		—,25	—,10	—,35	—,05
14	—,65		—,43	—,21	—,64	—,01
15	—,81		—,51	—,29	—,80	—,01
16	—,75		—,43	—,35	—,78	,03
17	—,60		—,25	—,41	—,66	,06
18	—,40	—3,61	,00	—,42	—,42	,02
19	—,07		,25	—,41	—,16	,09
20	,07		,43	—,35	,08	—,01
21	,17		,51	—,29	,22	—,05
22	,17		,43	—,21	,22	—,05
23	,07		,25	—,10	,15	—,08
24	,00	0,41	,00	,00	,00	,00

$$
\begin{aligned}
-3{,}74\,\text{c} - 4{,}29\,\text{d} &= -3{,}61 \\
3{,}74\,\text{c} - 3{,}29\,\text{d} &= 0{,}41 \\
\hline
\end{aligned}
$$

$\text{d} = 0{,}42;\quad \text{c} = 0{,}51.$

SAND KEY, *April* 8, 9, & 21.

Moon's declination N. 21° 08′;	Moon's transit, 5^h 54^m;
.. N. 20 48;	.. 6 52;
.. S. 21 17.	.. 17 37.

(TABLE NO. II.—DIAGRAM NO. 2.)

Hours.	Observed.		C. 0,45.	D. 0,53.	Computed.	O — C.
0	,00		,00	,00	,00	,00
1	—,10		—,22	,13	—,09	—,01
2	—,18		—,39	,26	—,13 L	—,05
3	—,10		—,45	,37	—,08	—,02
4	,03		—,39	,46	+,07	—,04
5	,30		—,22	,48	,26	,04
6	,60	0,55	,00	,53	,53	,07
7	,77		,22	,48	,70	,07
8	,86		,39	,46	,85 H	,01
9	,83		,45	,37	,82	,01
10	,63		,39	,26	,65	,02
11	,35		,22	,13	,35	,00
12	,00	3,44	,00	,00	,00	,00
		3,99				

$$D = 3{,}99 \div 7{,}58 = 0{,}53;$$
$$C = 1{,}70 \div 3{,}74 = 0{,}45.$$

Hours.	Observed.		C. 0,41.	D. 0,46.	Computed.	O — C.
12	,00		,00	,00	,00	,00
13	—,29		—,20	—,12	—,32	,03
14	—,55		—,35	—,23	—,58	,03
15	—,71		—,41	—,32	—,73	,02
16	—,75 L		—,35	—,39	—,74 L	—,01
17	—,66		—,20	—,45	—,65	—,01
18	—,46	—3,42	,00	—,46	—,46	,00
19	—,26		,20	—,45	—,25	,01
20	—,07		,35	—,39	—,04	,03
21	,05		,41	—,32	,09	—,04
22	,12		,35	—,23	,12 H	,00
33	,07		,20	—,12	,08	,01
24	,00	—0,09	,00	,00	,00	,00
		—3,51				

$$D = 0{,}46; \quad C = 0{,}41.$$

II. ZERO OF DECLINATION OF MOON.

SAND KEY, *January* 22 & 23, 1851.

Moon's declination N. 1° 01′; Moon's transit, $17^h\ 19^m$;
.. N. 3 53. .. 18 08.

(TABLE NO. III.—DIAGRAM No. 3.)

Hours.	Observed.		C. 0,50.	D. 0,03.	Computed.	O — C.
0	,00		,00	,00	,00	,00
1	—,25		—,25	,01	—,24	—,01
2	—,50		—,42	,01	—,41	—,09
3	—,53 L		—,50	,02	—,48	—,05
4	—,38		—,42	,03	—,39	,01
5	—,22		—,25	,03	—,22	,00
6	+,09	—1,86	,00	,03	—,03	,06
7	,23		,25	,03	,28	—,05
8	,48		,42	,03	,45	,03
9	,54		,50	,02	,52	,02
10	,48		,42	,01	,43	,05
11	,32		,25	,01	,26	,06
12	,00	2,04	,00	,00	,00	,00
		—0,18				

$$D = 0,18 \div 7,53 = 0,03;$$
$$C = 1,99 \div 3,74 = 0,50.$$

Hours.	Observed.		C.	D.	Computed.	O — C.
12	,00		,00	,00	,00	,00
13	—,27		—,25	—,01	—,26	—,01
14	—,56		—,42	—,02	—,44	—,12
15	—,61		—,50	—,03	—,53	—,09
16	—,46		—,42	—,04	—,46	,00
17	—,19		—,25	—,05	—,30	,11
18	,00	—2,09	,00	—,05	—,05	,05
19	,17		,25	—,05	,20	,03
20	,39		,42	—,04	,38	,01
21	,51		,50	—,03	,47	,04
22	,44		,42	—,02	,40	,04
23	,24		,25	—,01	,24	,00
24	,00	1,75	,00	,00	,00	,00

SAND KEY, *April* 1, 14 & 29.

Moon's declination S. 0° 58′; Moon's transit, 0^h 12^m;
.. N. 2 27; .. 11 29;
.. N. 2 05; .. 23 35+.

(TABLE NO. IV.—DIAGRAM NO. 4.)

Hours.	Observed.		C. 0,77.	D. —0,01.	Computed.	O — C.
0	,00		,00			,00
1	—,42		—,38			—,04
2	—,67		—,67			,00
3	—,75		—,77			,02
4	—,65		—,67			,02
5	—,42		—,38			—,04
6	,00	—2,91	,00			,00
7	,36		,38			—,02
8	,63		,67			—,04
9	,75		,77			—,02
10	,71		,67			,04
11	,36		,38			—,02
12	,00	2,81	,00			,00
		—0,10				

D = —0,10 ÷ 7,58 = —0,01;
C = 3,25 ÷ 3,74 = 0,77.

Hours.	Observed.		C. 0,74.	D. 0,04.	Computed.	O — C.
12	,00		,00	,00	,00	,00
13	—,36		—,37	—,01	—,38	,02
14	—,66		—,63	—,02	—,65	,02
15	—,84		—,74	—,03	—,77	,03
16	—,74		—,63	—,03	—,66	,03
17	—,34		—,37	—,04	—,41	,04
18	,00	—2,94	,00	—,04	,04	,04
19	,33		,37	—,04	,33	,04
20	,63		,63	—,03	,60	,03
21	,73		,74	—,03	,71	,03
22	,58		,63	—,02	,61	,02
23	,34		,37	—,01	,36	,01
24	,00	2,61	,00	,00	,00	,00

31. An Account of Longstreth's Lunar Formula. By Prof. B. Peirce, of Harvard.

Prof. Peirce stated that the title of his paper was probably sufficient to tell what he meant to say; but he wished it distinctly understood that he only intended to give an account of a discovery by a man who was as remarkable for his extreme modesty, as for the eminence of the position which he occupied among the scientific benefactors of the age. This was intended for an account of Mr. Longstreth's discovery, and was not his own. The very modest manner in which Mr. Longstreth had announced his discovery, was worthy of remark. He would read, from the preface to the published tables, all that Mr. Longstreth had himself said in relation to this great discovery. It was as follows: "The coefficients deduced from theory by Damoiseau, Plana, Pontecoulant, and those deduced from observation by Burckhardt (though differing considerably), give the moon's place with nearly the same accuracy. Where a difference exists, I have carefully compared them with observation, and deduced the most probable value." Longstreth had obtained results which involved the true theory of the variations of the moon's longitude. The results of observations, now that we had them in a tabular formula, would be sure to be confirmed by subsequent theory.

The professor exhibited the tables themselves, showing where Damoiseau and Plana agree, and where they differ; and stated that Prof. Airy, of England, had compared the results obtained by Plana's theory with observation. Longstreth's terms are often a return to the theories of Damoiseau and Laplace. The difference is generally greater between Plana and Laplace, than between Laplace and Longstreth. We are therefore travelling backward to the theory propounded by Laplace, while the supposed advances made by later physical astronomers are assuming their true position.

Mr. Longstreth's terms will be used in the American Nautical Almanac. This alone renders that work of the utmost importance to navigators of every nation, as well as of this country.

In conclusion, Prof. Peirce read the following letter, written by Mr. Longstreth in answer to a request of Lieut. Davis, superintendent of the Nautical Almanac, for a copy of his observations on the lunar motions:

PHILADELPHIA, 7th mo. 25, 1851.

CHARLES HENRY DAVIS, *Cambridge, Mass.*

Thy letter of the 20th ult. was duly received, and I have been prevented from answering it. I herewith inclose the coefficients used by me in the paper referred to. I selected the years 1820, '21 and '23 for comparison, as the errors of Plana's coefficients are the largest; and to avoid the charge of unfairness, I have retained all the observations made during those years; and where there is reason to question the correctness of any of them, I have mentioned it. In most of the cases where the errors are large, thee will find by reference to the Greenwich observations that no moon-culminating stars were observed. The years 1824 and '25 are a fair average of errors; and I would call thy attention to the smallness of the errors after the transit instrument was remounted (May 28, 1825). The smaller corrections of Airy I have not made, as the observations are not sufficiently accurate to justify it. I should be glad to hear from thee at thy leisure.

With much respect, thy friend,

M. FISHER LONGSTRETH.

V. METEOROLOGY AND METEORITES.

32. ON THE INFLUENCE OF TERRESTRIAL ELECTRICITY ON CLIMATE. By DANIEL VAUGHAN, of Cincinnati.

AN abstract of this paper was presented by Prof. HENRY, who stated that Mr. VAUGHAN had attempted to prove that there was a current of thermo-electrity which circulated around the earth in the same direction as the sun; and that when there was a change in latitude of isothermal lines, there was an interruption or break in the thermo-electric current. The origin of this supposed current was referred to the unequal heating of the earth's surface by the rays of the sun, which, passing in the course of twenty-four hours entirely around the earth, produces a continuous current in the same direction.

33. On the Distribution of Rain for the Month of September. By Prof. Elias Loomis, of New-York.

About twelve years ago I made a somewhat extensive comparison of meteorological observations, for the purpose of testing various popular notions with regard to the weather. My object particularly was to determine whether any connection could be traced between the fall of rain and the phases of the moon, or the seasons of the year. The result of this investigation was, that many popular proverbs with regard to the fall of rain had little foundation in truth. As, however, the materials from which this conclusion was derived were quite bulky, I did not offer them for publication; but having noticed that educated men have very extensively adopted many of these popular errors, I have concluded to bring forward some of the documents I have collected with reference to one question, viz. the general belief of an unusual fall of rain about the time of the autumnal equinox. The belief in an equinoctial storm appears to have prevailed not only in this country, but very extensively in Europe for a long period. In Nash's Mathematical Diary for 1820, published at New-York, the following question is propounded for solution: "Required the reason *why* storms are generally prevalent about the times of the equinoxes?" I propose to inquire whether rain *is* unusually prevalent about the time of the autumnal equinox.

The register to which I first refer for an answer to this question, is that kept by the Royal Society of London. This register was commenced in the year 1774, and was suspended at the close of the year 1843. There was also an interruption of six years from 1781 to 1786; being 64 years of observations. I have accordingly arranged the fall of rain for each day of September during this entire period in appropriate columns, and taken the sums for each day of the month. The amounts for the several days exhibit considerable inequality; the greatest being 5,649 inches on the 24th, and the least being 1,990 inches on the 3d; the greatest amount being almost three times the least. It cannot be doubted that this inequality is partly owing to the shortness of the period embraced in the observations; and we shall eliminate in some degree the influence of causes which are independent of the day of the month, by grouping the observations. If we take the sums of the daily amounts for intervals of five days, we shall obtain the numbers

18,403 inches;
20,835 ..
19,244 ..
19,354 ..
23,075 ..
22,448 ..

Here the numbers approach much nearer to equality, the greatest sum exceeding the least by about one quarter of its whole amount. If we take the sums for the two halves of the month, we shall find that the amount for the last half is about one fifth greater than for the first half. This last result being deduced from a comparison of 1920 days, must be regarded as indicating a law of the climate of London.

In order to determine whether this result has any particular connection with the equinox, let us compare the average fall of rain at London for successive months.

According to the observations of Mr. Howard, the average fall of rain in September is 1,921 inches; in October, 2,522 inches; in November, 2,998 inches; that is, the fall of rain in October is one third greater than in September. This result is in perfect harmony with that before discovered, viz. that the fall of rain for the last half of September is one fifth greater than for the first half. We perceive then a fluctuation in the amount of rain, depending upon the season of the year; but no decisive indication that the rain for any day of September will be found uniformly greater than on both the preceding and following days.

We shall arrive at the same result, if we make the comparison by the *number* of rainy days, instead of the *quantity* of rain. The greatest number of rainy days for the entire period, occurring on the same day of the month, was 28, being on the 27th; the least number was 15, on the 3d. If we classify the numbers in groups of five days, we obtain the following result:

110,
96,
94,
97,
117,
112.

Here the greatest sum, as before, exceeds the least by one quarter of its whole amount. The total number of rainy days corresponding to the first half of the month was 300; for the last half, 326; being an excess of about one twelfth of the whole number. We see then

the influence of the annual fluctuation in the increased number of rainy days for the last half of the month; but no decisive indication that any day of September could be selected as more exposed to rain than both the preceding and following days.

I will not attempt to conceal that the amount of rain for the five days embracing the equinox is greater than for any other period of five days throughout the month; but if any one should be disposed to attach any special importance to this circumstance, I would remark that the amount of rain for the last five days of the month falls short of the amount for the preceding five days by less than three per cent, and that this quantity is too small to afford any satisfactory basis for a conclusion in a research of this kind. Certain it is that the difference is too small to be detected without a most careful observation of the rain-gauge; and inasmuch as the popular belief on this subject was certainly never derived from meteorological journals, I do not hesitate to conclude that the common opinion of an unusual fall of rain at London about the time of the autumnal equinox has been taken up without reason.

I now proceed to inquire what foundation there may be for a similar opinion in the United States. And here we encounter a difficulty, arising from the want of a continued register of the fall of rain at a single locality for any long period. Most of the early meteorological observers paid little attention to the amount of rain. I have been unable to find any register in this country, furnishing the amount of rain for more than about a quarter of a century. I have accordingly taken for comparison the observations made at Albany under the direction of the Regents of the University of the State, embracing a period of 24 years. The greatest sum for any one day of the month of September, for the entire period, is 7,18 inches, on the 3d; the least sum is 0,97 inches, on the 17th. The inequalities from day to day are so striking, as to indicate clearly that the period of comparison is too short to eliminate the effect of any causes independent of the day of the month. Let us then group the observations in periods of five days, and we shall obtain

20,62 inches;
8,81 ..
13,34 ..
13,82 ..
17,16 ..
13,48 ..

The first five days of the month furnish the greatest amount of rain, and the next five days the least amount. So far then as these observations indicate any influence of the day of the month upon the amount of rain, they would lead to the conclusion that an unusual fall of rain is to be expected about the 3d of September; and that from the 6th to the 10th of the month, it will be unusually dry. In point of fact, I consider such a conclusion premature. Certainly the observations afford no countenance to the impression that an unusual fall of rain is to be expected about the time of the equinox. It is worthy of note, that for 23 successive years, no rain is recorded as having fallen on the 6th of the month; so that one year ago, it would have been a legitimate inference from these observations, that on the 6th of September it never rains at Albany. This circumstance plainly indicates the necessity of extending our observations over a long period of time, if we would entirely eliminate the influence of accidental causes. It is also suggested that probably no citizen of Albany has ever noticed this remarkable absence of rain for 23 years; and still I doubt not there are thousands who will tell us that they have always noticed, that if it rains on the first sabbath of a month, it is sure to rain on each succeeding sabbath.

In order to supply as far as possible the want of a series of observations sufficiently long, I have had recourse to the Journal of Dr. HOLYOKE, kept at Salem (Mass.) from 1786 to 1828; in which it was recorded each day whether it was fair, cloudy, or rainy, although the amount of rain was not registered. I have taken the sum of the rainy days for each day of the month, and have appended to this the Albany register, making thus a continued record for 65 years. The greatest number of rainy days on any one day of the month, for the entire period, was 25 on the 5th; and the least number was 12, on the 6th. In order to eliminate the influence of foreign causes, we will group the observations in periods of five days, and we obtain the following numbers:

98	rains;
76	..
90	..
86	..
89	..
91	..

So far then as these observations indicate any influence of the day of the month upon the amount of rain, they lead to the conclusion that the first five days of September are unusually rainy, and the second five days unusually dry. In point of fact, I regard such a conclusion as premature; but the observations clearly indicate that there is no more reason to expect rain at the time of the autumnal equinox, than on the week preceding or the week following.

From all these observations, I infer that the popular notion of an unusual fall of rain, either in Old England or in New England, about the time of the autumnal equinox, is wholly unfounded; and is akin to those superstitions which make some particular day of the month "observation day" for the entire month, or make the fall of rain dependent upon the annual meeting of Quakers; or, in the language of the Poet,

> If the first of July be rainy weather,
> 'Twill rain more or less for forty days together.

	23	24	25	26	27	28	29	30
1774	,244	,235	,695	,108	,076	,019	,236	,264
5	.....	.....	.....	.....	.....	.....	.....	.....
6	.....	.....	.....	,131	.....	,019	.....	.....
7	.....	.....	.....	.....	,220	.....	.....	,078
8	.....	.....	.....	.....	,100	,033	.....	.....
9	,140	,945	,030	,045	,227	.....	,036	.....
80	.....	.....	,113	.....	,079	.....	,320	,032
7	,017	.....	,098	.....	.....	.....	.....	.....
8	,145	,302	,062	.....	,058	.....	,190	.....
9	.....	.....	.....	.....	.....	,075	.....	,353
90	.....	,063	.....	.....	.....	.....	.....	.....
1	.....	.....	.....	.....	.....	.....	.....	.....
2	,043	,017	,014	,028	.....	,040	,071	,167
3	,099	,098	,141	.....	.....	.....	.....	.....
4	.....	,187	,094	,035	.....	.....	.....	.....
5	.....	.....	.....	.....	,020	.....	.....	.....
6	,116	,018	.....	,320	.....	.....	.....	.....
7	.....	.....	,270	,410	.....	.....	.....	,085
8	.....	.....	.....	.....	,115	,730	,321	.....
9	,248	.....	.....	.....	,210	.....	,478	,363
1800	.....	.....	,241	.....	,058	,202	,072	,032
1	,086	,197	.....	.....	,115	.....	.....	.....
2	.....	.....	.....	.....	.....	.....	.....	.....
3	.....	.....	.....	.....	.....	.....	.....	.....
4	.....	.....	.....	.....	.....	.....	.....	.....
5	,046	.....	.....	.....	.....	.....	.....	.....
6	.....	.....	.....	.....	.....	.....	.....	,475
7	,443	,202	.....	,270	,122	,082	.....	,016
8	,335	.....	.....	.....	.....	.....	.....	.....
9	.....	,110	,085	,420	,060	,180	,090	.....
10	.....	.....	.....	.....	.....	.....	.....	.....
1	,140	,140	,460	,100	,450	.....	,200	,160
2	.....	.....	.....	.....	,040	.....	.....	.....
3	,140	.....	.....	.....	.....	.....	.....	.....
4	.....	.....	,350	.....	,310	.....	.....	.....
5	.....	,260	.....	,230	,130	.....	,340	,170
6	.....	,130	.....	.....	.....	.....	,480	.....
7	.....	.....	.....	,200	,070	.....	.....	.....
8	,200	.....	1,220	,200	.....	.....	.....	,250
9	.....	,030	,330	,110	,400	,660	,230	.....
20	,050	.....	.....	.....	,140	,120	.....	,030
1	.....	.....	.....	,170	.....	,300	.....	,020
2	,290	,850	.....	.....	.....	.....	.....	.....
3	.....	,272	.....	.....	.....	.....	.....	,270
4	,093	,148	,008	.....	,075	.....	.....	.....
5	.....	.....	.....	,562	.....	.....	.....	.....
6	.....	.....	.....	.....	.....	.....	.....	.....
7	.....	.....	.....	.....	.....	.....	.....	,093
8	.....	.....	.....	.....	,250	,285	,203	,233
9	,019	.....	.....	.....	.....	,044	.....	.....
30	.....	.....	,250	.....	.....	.....	,250	.....
1	.....	.....	.....	,033	.....	,325	,631	.....
2	.....	.....	.....	.....	.....	.....	.....	,047
3	.....	.....	.....	.....	.....	.....	.....	.....
4	.....	.....	.....	.....	,055	,092	.....	.....
5	,111	,250	.....	.....	,788	,055	.....	.....
6	,044	,161	.....	.....	.....	,047	,486	,255
7	.....	.....	.....	.....	.....	.....	.....	.....
8	.....	,038	,750	.....	,116	,783	.....	,050
9	,044	.....	,055	,016	.....	.....	.....	.....
40	,333	.....	,097	,019	,130	.....	,269	.....
1	,602	,366	,158	,080	,327	,438	,088	,366
2	.....	,630	,111	,338	,038	,288	.....	.....
3	.....	.....	.....	.....	.....	.....	.....	,238
64 y'rs	4,028	5,649	5,632	3,820	4,829	4,816	4,941	4,042
	75 inches;				22,448 inches.			
	s, and 326 rainy days.							
	24	23	22	21	28	21	19	23
	days of rain.				112 days of rain.			

TABLE NO. III.

DR. HOLYOKE'S JOURNAL, KEPT AT SALEM (MASS.), 1786 – 1823;

SHOWING THE RAINY DAYS EACH SEPTEMBER FOR 43 YEARS.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
1786	..	..	..	..	1	1	..	..	..	..	1	1	..	..	..	..	..	1	1	..	..	..	..	..	..	..	..	..	..	..
87	..	1	..	..	1	..	..	..	..	1	1	..	..	1	..	..	..	..	1	1	..	1	1	1	..	..	1	..	1	..
88	1	..	..	..	..	..	1	..	..	..	..	..	..	..	..	1	..	..	1	..	..	..	..	..	..	..	..	..	..	1
89	1	1	1	..	..	..	..	..	..	..	..	..	1	..	1	..	..	..	..	..	..	..	..	..	..	1	1	..	1	..
1790	..	..	..	1	..	1	..	..	..	..	..	..	..	1	..	..	..	..	..	..	..	1	1	..	..	..	1	1	..	1
91	1	1	..	..	1	1	..	..	..	1	1	1	..	1	1	..	..	..	..	..	..	..	..	1	1	..	1	1	..	..
92	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	1	1	..	..	..	..	..	..	..	..	1	1	..	..	..
93	..	..	..	..	1	..	..	..	1	1	1	1	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..
94	1	1	..	1	..	..	1	..	..	..	..	..	..	..	..	1	1	..	..	1	..	..	..	..	..	..	1	..	1	..
95	1	1	..	..	..	..	1	1	..	..	1	..	1	..	..	1	1	..	..	1	1	..	..	..	..	..	1	1	..	..
96	..	1	1	..	..	..	..	..	1	..	..	1	..	1	..	..	1	..	..	..	..	1	..	..	..	..	..	1	1	..
97	..	..	..	..	..	1	1	..	..	..	..	1	..	..	1	..	..	..	1	..	..	..	..	..	..	1	..	..	..	..
98	..	..	..	1	1	..	1	..	..	..	..	..	..	..	..	..	..	..	1	..	1	..	..	..	1	..	..	..	..	..
99	1	..	..	1	1	1	1	..	..	1	..	..	..	1	..	..	..	..	..	..	..	1	..	1	..	..	..	..	1	..
1800	..	1	..	1	..	..	..	..	..	..	..	..	..	1	..	1	1	..	..	..	1	1	1	..	..	1	1	1	..	..
1	..	..	..	1	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..
2	..	..	1	..	..	..	..	..	1	1	..	..	..	1	..	..	1	..	..	..	..	1	1	1	..	..	..	..	..	..
3	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	1	..	..	..	1	..	..	..	..	..	1	..	..	..	..
4	..	1	..	..	..	..	..	..	..	..	1	1	1	..	..	..	1	1	1	1	..	..	..	..	..	1	..	..	..	..
5	..	..	..	..	..	..	..	..	..	..	..	1	1	1	1	..	..	..	1	..	..	..	1	..	..	..	..	..	1	..
6	1	1	1	..	1	1	..	1	1	..	..	1	1	..	..	..	..	..	1	..	..	..	..	..	..	1	..	..	..	1
7	..	1	..	..	1	..	..	1	..	..	..	..	1	1	..	..	..	..	1	..	..	..	1	..	..	1	..	..	..	..
8	..	..	..	1	..	1	..	..	..	..	..	..	..	..	..	..	1	..	1	1	..	..	..	..	1	..	..	..	1	1
9	..	..	..	1	..	..	..	..	..	..	..	..	..	..	1	1	..	..	..	..	..	..	1	..	1	..	..	..	..	..
1810	..	..	..	1	..	..	..	1	..	..	..	..	..	..	1	..	..	..	..	1	1	..	1	1	1	1	..	..	..	..
11	..	..	..	..	1	..	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	1	..	..	1	..	..	..	1	..
12	..	..	..	..	1	1	..	..	..	..	..	..	..	1	..	..	..	1	..	1	..	1	..	..	1	..	..	..	..	1
13	..	..	1	..	..	..	..	..	1	..	..	..	..	..	..	..	1	1	..	..	..	..	1	..	..	..	..	..	1	1
14	..	..	..	..	..	..	1	..	..	..	..	1	1	1	..	1	1	..	..	..	..	1	1	..	..	..	..	..	..	..
15	..	..	1	..	1	1	..	..	..	..	1	1	1	1	..	1	1	1	..	..	..	1	1	..	..	..	..	..	..	..
16	..	..	..	..	..	..	..	..	..	..	..	..	1	1	1	..	..	..	1	1	..	..	..	1	..	..	..	..	..	..
17	..	:	..	..	..	..	..	..	..	..	..	1	..	..	..	1	..	1	..	..	..	1	..	1	..	..	..	1	..	..
18	..	..	..	..	..	..	1	1	1	..	..	..	..	1	1	1	1	..	1	..	..	..	..	..	1	..	..	..	..	..
19	..	..	..	..	..	..	1	..	..	..	..	..	1	..	..	..	..	..	..	..	1	1	1	1	1	..	..	..	1	..
1820	..	..	..	..	..	..	..	..	..	..	..	1	..	..	1	1	..	1	..	..	..	..	..	1	..	..	..	1	1	..
21	1	1	1	1	..	..	..	1	1	..	1	1	..	..	..	1	1	..	..	..	1	..	..	1	..	..	..	..	..	..
22	..	..	1	..	..	..	1	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	..	..	1	..
23	..	..	..	1	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	1	1	..
24	1	..	..	1	..	1	1	1	..	..	..	1	1	..	..	..	1	..	..	..	..	1	..	..	..	..	..	1	1	1
25	..	..	1	1	1	..	..	1	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	1	..	..	1	..	..	..
26	..	..	1	1	1	..	..	..	..	1	..	1	..	..	..	..	..	1	..	..	..	..	1	1	..	..	1	..	..	1
27	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	..	1	1	1	1	1	1	..	..	..	..	..	..	..
1823	..	1	1	1	1	..	..	..	1	1	..	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..	..	1	..
43y	9	12	11	15	14	11	11	8	9	10	8	16	11	14	9	13	16	9	13	11	7	14	14	12	9	12	10	9	15	8

72 (days 1–6), 62 (days 7–12), 72 (days 13–18), 71 (days 19–24), 63 (days 25–30)

First half 168. Last half 172.

61 (days 1–5), 49 (days 6–10), 58 (days 11–15), 62 (days 16–20), 56 (days 21–25), 54 (days 26–30)

ALBANY, 1829 to 1850 : 22 YEARS.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
	4	7	7	8	11	1	6	8	6	6	8	5	6	8	5	3	3	10	4	6	8	3	5	9	8	6	9	3	7	12
65y	13	19	18	23	25	12	17	16	15	16	16	21	17	22	14	16	19	19	17	17	15	17	19	21	17	18	19	12	22	20

98 (days 1–5), 76 (days 6–10), 90 (days 11–15), 88 (days 16–20), 89 (days 21–25), 91 (days 26–30)

First half 264. Second half 268.

Mr. W. C. Redfield remarked, that nothing could be more unfounded than the traditional notion of an immediate or special connection between the equinox and the storms of that period. The meteorological records which he had examined, afford no grounds for this very prevalent opinion; as will equally appear, whether we view the fall of rain as alone constituting the storm, or consider the rain as but a common though not essential feature of a storm or gale.

(In further discussion on Prof. Loomis's paper, Dr. Hare referred to another paper of Prof. Loomis on the storm of December 1836, as proving the progress of that storm towards the southeast, and thus far confuting the views of Mr. Redfield.)

Mr. Redfield said, in regard to the storm of December 1836, so elaborately investigated by Prof. Loomis, its alleged line of progress towards the southeast he deemed as not accordant with the barometric observations which Prof. Loomis had adduced. For if the true line of progress be that of the area of greatest barometric depression in the storm, as Dr. Hare justly admits, it will be found from the barometric observations at Syracuse, Montreal and Quebec, when compared with those east of the Hudson and at Halifax, that the actual progress of the barometric minimum or nucleus of the storm was towards the *northeast*, passing over the valley of the St. Lawrence in its course. This greatly affects some of the inductions regarding the phenomena of that storm.

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34. Strictures on Professor Espy's Report on Storms to the Secretary of the Navy, as respects the Theoretical Inferences. By Dr. Robert Hare.

I have seen a report made to the Secretary of the Navy by Prof. Espy, which, so far as it correctly records the phenomena of various storms, must be honorable to the author, and worthy of the department of our government under whose auspices it has been made. Doubtless, in this, as in other publications originating from the same source, there may be a great exhibition of ability, science, and zeal: nevertheless, I question the propriety of making any particular hypothesis the subject of an official report by its author, an ardent advocate, unaccompanied by a fair summary of the objections which

have been made to it, or any notice of any other hypothesis which may have been advanced as preferable. With Prof. Espy's opinions I concur so far as to agree in the inference that hurricanes and tornadoes are the consequence of the ascent of air from a focal area or intermediate space, by which a confluence from two or more opposite quarters, to supply the deficit thus arising, is induced; yet we differ as to the cause of the ascent of the air in such cases. In the year 1835 I advanced, before a meeting of the American Philosophical Society, that the cause of the ascent in question was a discharge of electricity between the earth and sky. This explanation was made the subject of a memoir published in the transactions of that society in the year 1836.

I will endeavor to give a sketch of the views which I now entertain on this subject, hoping to present them more briefly and forcibly than I did at that time.

Every person familiar with the phenomena of electricity as produced by an electrical machine, must be aware that there are two modes in which a discharge may be effected between the oppositely charged surfaces of conductors, or of a coated electric. In one case, simultaneously with the discharge, a vivid spark is seen to take place; in the other case, some movable body, such as a bell-clapper, a pith-ball, or a blast of air issuing from a projecting point, is made to convey electricity from one surface to the other, until a discharge is accomplished. The latter process has been designated by Faraday as the convective discharge, from *conveho*, to carry; while the former is designated as diruptive, from *dirumpo*, to break through; since, in this case, the opposite waves break through the air, conveying the whole charge at once; while in the other process, the opposite excitements are gradually neutralized by successive contacts with the matter passing from one surface to the other. Notoriously either of these discharging processes may be substituted for the other, by a slight variation of distance.

Thus, in the experiment in which pith-balls are made to resemble hail, by dancing between oppositely electrified disks, an approximation of one of the disks towards the other induces a spark or diruptive discharge, and thus causes dancing to cease. In Cuthbertson's balance electrometer, the movable ball approaches that which is stationary, in obedience to the convective process; but as soon as the distance between the balls is reduced within the striking distance, a diruptive discharge ensues, indicated as usual by a spark.

It follows that by a slight variation as to distance, the same degree of electrical excitement may be productive either of a convective or of a diruptive discharge. Excepting a prodigious disparity in magnitude, the diruptive spark discharge is universally recognized as perfectly similar to lightning : both are admitted to be due to discharges of electrical accumulations, differing only as to magnitude. Since, agreeably to this exposition, susceptibility of commutation exists as respects diruptive discharge in its minuter forms and convective discharge upon the same scale, does it not follow that the former, as produced by the gigantic processes of nature, should be commutable with a convective process of corresponding immensity? But if the spark or diruptive discharge is exemplified by lightning, how is the latter to be exemplified? Where is there any gigantic meteorological process which can supply the deficiency, excepting that of the tornado or hurricane, which last may be viewed as a tornado on a scale of preëminent grandeur?

If from a point electrified by a machine, a blast of air may proceed as strong as from a blowpipe supplied by a bellows, may not an enormous blast be emitted from every terrestrial prominence electrified by the powerful apparatus of nature, as much greater than that of a blowpipe, as a spark of lightning of a mile in length exceeds that yielded by an excited conductor or charged jar? So long as there is an ascent of air consequent to electrical convection, there must be a confluence of the same fluid from two or more opposite quarters to supply the deficit thus created; and the air, as it follows the electrified column, being successively similarly electrified, that enduring trunk or column is formed and sustained which characterizes tornadoes or waterspouts.

Within this travelling trunk, which, in its form, contortions and deleterious power, resembles that of an enormous elephant, as mischievous as gigantic, bodies are not only subjected to the same convective influence as the air, but are also exposed to the upward force arising from a vertical blast. On each side of the track which marks the progress of the trunk, bodies are subjected to the confluent blasts which rush in to supply the upward current.

The alternation of the convective and diruptive discharges was well exemplified in the phenomena of the Providence Tornado of 1840, as described by a most worthy and well-informed observer, Zachariah Allen, Esq. As soon as the trunk reached the river, the water throughout the included area rose up as in a state of ebullition

by the convective influence; but a diruptive discharge in the form of lightning taking place, the foam subsided momentarily, yet rose again, until by another spark of lightning another subsidence ensued. Were ever facts more accordant with an explanation, than those observed by Mr. Allen with the hypothesis which I advanced?

Hurricanes may be considered as the consequence of a convective electrical discharge, on a vastly more extensive scale than tornadoes. Evidently there can be no conceivable limits to the immensity which such electrical discharges may acquire. All that is essential to an accumulation of electricity analogous to that which may be secured by means of a coated pane or leyden jar, is, that there shall be a suitable electric to fill the office performed by the glass in those instruments, and two conductors competent to act as coatings.

Experience shows that the denser portion of the atmosphere, which lies between the storm-clouds and the earth, is competent to act as an electric; since otherwise there would be no thunder-gusts, nor any atmospheric discharges as displayed in the form of lightning. That air, rarefied to a certain degree, becomes capable of acting as a coating does in the instance of the leyden jar, is proved by the fact that the inner surface of a glass globe, within which the air is rarefied by exhaustion, may be charged like a leyden jar, if to the outer surface a conducting body be applied, and a due communication made with an electrical machine in operation.

As it is well known that the terrestrial surface is a conductor, it follows that in that surface, the denser air in proximity therewith, and the rarefied conducting air above, we have an electric between two conductors competent to act as coatings. Thus the dense air acts as a glass pane between two coatings, or as the glass in an exhausted globe acts between the rarefied air within and the hand of the operator without. We have, therefore, all that is requisite to the reception of an electrical charge.

That the means of disturbing of the electric equilibrium are abundantly prolific, the terrific discharges of lightning in electrical storms can leave no doubt.

Using the language of the Franklinian theory, I urged that, in the concentric spaces occupied by the earth and that occupied by the rare conducting medium above alluded to, there must be two oceans of electricity, which could not fail from mechanical or chemical causes to be in different states. But assuming that electricity is a result of the polarization of the ethereal fluid, to the undulation of

which light is ascribed, we are led to substitute for oceans of a specific fluid, the idea of a boundless ocean of ethereal matter, which by peculiar affections may become competent to perform, within the concentric spaces alluded to, the part assigned by Franklin to one fluid, by Dufay to two fluids.

Consistently it may be inferred that an atmospheric change may extend all around the globe, so as to make one great battery analogous to that above described of the exhausted glass globe; the rarefaction being in one case internal, in the other external. Agreeably to these considerations, there are no limits to the possible extent of atmospheric accumulations of electricity, while the rapidity with which discharges pervade conductors is such as to render distance no obstacle. Agreeably to the lowest estimate of the velocity of the electric waves as produced by a galvanic apparatus (of a very low intensity compared with frictional accumulations), in two seconds the waves would encompass the earth; but according to Wheatstone, a discharge from a leyden jar would, during the same time, go round the globe ten times.

Against the idea that there could be any adequacy in the apparatus of nature, such as to make bodies dance between the earth and sky, as puppets and pith-balls are seen to dance between electrified brass disks; it was some time since objected, by a distinguished meteorologist, that a stratum of an elastic fluid, like air, could not perform the part of a solid metallic disk.

The answer to this is, that whatever state of things is competent to sustain electrical charges, is competent to produce *any* of the phenomena of *dis*charges. Just as much stability is requisite to enable the diruptive discharge of lightning to take place, as to enable the convective discharge of the tornado or waterspout.

The descent of the ball, in the operation of Cuthbertson's electrometer, before emitting a spark, shows that attraction accompanies both discharges; since convection, causing the partial descent of the ball, takes place, as above described, before a spark ensues.

There can be no doubt, that so far as electrical repulsion counteracts gravitation during a convective discharge, the air must be released from a portion of the compression to which it is usually subjected. There must, therefore, be a sudden dilatation coöperating with the other causes of violence.

From all that I have urged, I infer that there is no necessity for our seeking other causes for electrical storms, than those which may

be found within the province of that all-important agent in the physical creation which we call electricity; and further, while it has been shown, I trust, that in atmospheric accumulations there is an ample source of stormy reaction in its most violent forms, I hope to prove that the cause assigned by Mr. Espy is incompetent to produce such violent reaction.

It is well known that, when suddenly rarefied, air is refrigerated: hence, when a receiver is first subjected to exhaustion, a cloud appears within it, arising from the condensation of aqueous vapor. Dalton found that when the air thus rarefied was devoid of aqueous vapor, it became much colder than when this vapor was present: this he ascribed to the latent heat given out by aqueous vapor on condensing. Before I had the pleasure of knowing Mr. Espy, I contrived an apparatus for showing the cloud and color produced by rarefaction.

This apparatus, as well as that employed by Dalton, does not differ essentially from Espy's nephiliscope, which is the name given by him to an instrument answering the same purpose as that employed by Dalton. Notoriously the density of the air diminishes in a geometrical ratio, as the place of examination is higher; so that at the altitude of three miles, it is only half as dense as upon the earth's surface.

Davy, in his Elements, ascribed the formation of clouds to the refrigeration arising from the rarefaction of ascending columns of air; and to this I used to advert in my lectures nearly thirty years ago, using the nephiliscope, which I had contrived as above mentioned, to illustrate the idea.

Thus it became evident, from the experiments and suggestions of Dalton and Davy, that when the different portions of air in an upward current successively reach a height sufficient to rarefy and cool them to a certain extent, the aqueous vapor which they hold must form a cloud, and at the same time render them lighter and warmer than the surrounding air.

It was first assumed by Espy that the rise of temperature thus caused would create a buoyancy like that of a balloon, and an upward force, and so great an acceleration as to produce the phenomena of a tornado at the foot of the column affected. In fact, the buoyancy thus arising is, by this ingenious author, considered as universally the cause of storms.

Admitting his estimate of the buoyancy consequent to the con-

densation of vapor to be correct, I aver that no buoyancy thus created in the upper part of an aerial column would cause any disturbance of the column below the level of that upper part.

Count Rumford first showed that water may be boiled at the top of a containing vessel, without warming or disturbing the liquid lying below the part where the heat may be applied. This fact has been demonstrated by me, on a large scale, during each of thirty courses of lectures. *In Mr. Espy's presence*, about five years ago, I demonstrated that this law is equally true in the case of air.

A large bell-glass was so supported in an inverted position, as to allow the axis of a spirit-lamp flame to be concentric with the bore of the neck. In the next place, a tuft of cotton, nearly equalling in diameter the mouth of the bell, was moistened with alcohol. By means of tongs, this tuft, being held just above the mouth of the bell, was inflamed. Of course, the difference of temperature thus created was incomparably greater than any which could be producible by the latent heat yielded by condensing vapor. Moreover, the whole lifting influence was concentrated upon the comparatively narrow area of the bore in the neck; yet the smallest acceleration could not be perceived to take place : the flame was not in the slightest degree disturbed. Subsequently, at the meettng of the Association at Cambridge in 1849, an apparatus was constructed, by which the experiment above described was repeated with an improved arrangement.

Inside of the inverted bell, so as to cover the bore of the neck immediately over which it rested, a disk of wire gauze was placed, supporting a few thin fibres of carded cotton. About half an inch above the mouth of the bell, another disk or tray of wire gauze was upheld by appropriate means, on which there was put a stratum of carded cotton sufficiently copious. These preparations being completed, the cotton above the bell was ignited. Notwithstanding the enormous rise of temperature thus produced in the upper part of the column of air, of which the lower portion occupied the bell-glass, so entirely was this lower portion uninfluenced, that there was not the least perceptible agitation produced among the most delicate fibres of the cotton.

This perfect immobility of the air subjacent to a column of that fluid, to which a great ascensional power seems to be imparted by the ignition of the cotton as above described, will not excite wonder, when it is recollected that the buoyancy is not the consequence of

absolute levity, but of comparatively lesser weight. The ascent of a balloon is not spontaneous; it is the effect of coercion : it is forced to ascend, by the superior gravity and consequent pressure of the surrounding air; but while this displaces the balloon, it does not on that account relax its pressure on the subjacent portion of the atmosphere.

It is admitted that, on reaching the rarefied region where the atmospheric clouds appear, the consequent condensation of aqueous vapor will make any body of air containing it warmer than it would otherwise be; and from the lowest level above which the heat is applied, there would be a more or less disturbance, in consequence of the greater buoyancy of the column warmed by the condensation of vapor. But this disturbance would, as I conceive, be much less abrupt and forcible than the Espian hypothesis of storms requires.

Even after the condensation of aqueous vapor is effected, the water which formed it will remain within the column, and still add to its weight, so that the total weight will not be diminished. Moreover, by swelling upwards, as it naturally will do, towards the region where there is least resistance, it will become as much taller as rarer, and thus compensate by its greater height for the loss of specific gravity. In a non-elastic fluid, any superiority of elevation, in any portion expanded more than the rest, would be rapidly compensated by the overflow of the excess; but in an elastic fluid, where the summit must be so rare as to have scarcely any perceptible weight, no such active overflow can take place as would be requisite to produce any violent exchange of position between the column thus affected and the surrounding portion of atmosphere.

If, as represented by Espy, all that is requisite to produce a tornado, is an upward current of air, preëminently warm and moist, and penetrating into the region of the clouds, the conditions are abundantly realized in the vicinity of the equator. The trade winds have long been ascribed to the ascent of air from the regions on each side of the equatorial line, in consequence of the rarefaction arising from a comparatively superior temperature. To supply the vertical current thus created, the air is conceived to flow towards the equator from regions more remote, and less heated by the sun; the currents thus caused being rendered more westerly in their directions relatively to the earth's surface, by the diurnal motion of that surface, which is necessarily accelerated with the increase of its distance from the terrestrial axis as the equator is approached. As, in con-

sequence of the warmth to which its ascent is attributed, and an ample contact with the surface, the upward current must be replete with aqueous vapor, all the requisites which the Espian theory requires for the production of a perpetual gigantic tornado are present; and yet none is produced.

With the hypothesis which ascribes tornadoes to an electrical discharge, it is quite consistent that there should be no thunder-storms within the region of the vertical current, or the trade winds produced thereby; since there is a perpetual discharge by convection, preventing of course any electrical accumulations.

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35. On the Clouds and the Equatorial Cloud Rings of the Earth. By Lieut. M. F. Maury, Superintendent of the National Observatory.

The sailor at sea observes phenomena, and witnesses operations in the terrestrial economy which tell him, that in the beautiful and exquisite adjustments of the grand machinery of the atmosphere, the clouds have important offices to perform besides those of dispensing showers, of producing the rains, and of weaving mantles of snow for the protection of our fields in winter. As important as is this office, the philosophical mariner — and there are many such — is reminded that the clouds have other commandments to fulfil, which, though less obvious, are not therefore the less benign or the less worthy of note. He beholds them at work in moderating the extremes of heat and cold, and in mitigating climates. At one time they spread themselves out; they cover the earth as with a mantle; they prevent radiation from its crust, and keep it warm : at another time, they interpose between it and the sun, and screen it from his scorching rays, to protect the tender plants from his heat, the land from the drouth. Having performed this office for one place, they are evaporated and given up to the sunbeam and the wind again, to be borne on their wings away to other places which stand in need of like offices.

Familiar with clouds and sunshine, the storm and the calm, and all the phenomena which the lightning and the blast present, the right-minded mariner, as he contemplates "the cloud without rain," ceases

to regard it as an empty thing; he perceives that it performs many important offices; he regards it as a great moderator of heat and cold, and considers the equatorial cloud-ring which encircles the earth as a great balance-wheel in the atmospherical machinery. Bound in his ship hence to the southern hemisphere, he enters the region of the northeast trades, and finds the sky sometimes mottled with clouds, but for the most part clear; continuing his course towards the line, he finds his thermometer to rise higher and higher as he approaches the equator; until entering the region of equatorial calms and rains, he feels the weather to become singularly murky, close and oppressive; he discovers here that the elasticity of feeling which he breathed from the trade wind air, has forsaken him.

Escaping from this gloomy region, and entering the southeast trades, his spirits revive, and he turns to his Log-book to see what changes are recorded there. He is surprised to find, that notwithstanding the oppressive weather of the rainy latitudes, both his thermometer and barometer stood, while in them, lower than in the clear weather on either side of them; that just before entering, and just before leaving the rainy parallels, the mercury of the thermometer and barometer invariably stands higher than it does when within them, even though they include the equator. He has passed a ring of clouds that encircles the earth.

Perceiving this, he is reminded how this cloud-ring, by screening these parallels from the sun's rays, not only promotes the precipitation which takes place within it at certain periods, but how also the rains are made to change the places upon which they are to fall; and how, by travelling with the calm belt of the equator up and down the earth, this cloud-ring shifts the surface from which the heating rays of the sun are excluded; and how, by this operation, tone is given to the atmospherical circulation of the world.

In a clear day at the equator, this cloud-ring having slipt to the north or south with the calm belt, the rays of the sun pour down upon the crust of the earth and raise its temperature to a scorching heat. The atmosphere dances above it, and the air is seen trembling in ascending and descending columns with busy eagerness to conduct the heat off, and deliver it to the regions aloft, where it is required to give momentum to the air in its general channels of circulation. The dry season continues; the sun is vertical; and finally the earth becomes parched and dry: the heat accumulates faster than the air can carry it away; the plants begin to wither, and the

animals to perish. Then comes the mitigating cloud-ring; the burning rays of the sun are intercepted by it. The place for the absorption and reflection, and the delivery to the atmosphere of the solar heat is changed; it is transferred from the upper surface of the earth to the upper surface of the clouds.

Radiation from the land, and from the sea below the cloud belt, is thus interrupted, and the excess of heat in the earth is delivered to the air, and by absorption carried up to the clouds, and there delivered to their vapors to prevent excess of precipitation.

In the mean time, the trade winds north and south are pouring into this cloud-covered receiver, as the calm and rain belt of the equator may be called, fresh supplies in the shape of ceaseless volumes of heated air loaded to saturation with vapor, which has to rise above and get clear of the clouds before it can commence the process of cooling by radiation. In the mean time also the vapors which the trade winds bring from the north and the south, expanding and growing cooler as they ascend, are being condensed on the lower side of the cloud stratum, and their latent heat is set free, to check precipitation and prevent a flood.

While this process and these operations are going on on the nether side of the cloud-ring, one not less important is going on on the upper side. There, from sunrise to sunset the rays of the sun are pouring down without intermission. Every day, and all day long, they operate with ceaseless activity upon the upper surface of the cloud stratum. When they become too powerful, and convey more heat to the cloud vapors than the cloud vapors can reflect and give off to the air above them, then, with a beautiful elasticity of character, the clouds absorb the surplus heat. They melt away, become invisible, and retain, in a latent and harmless state, until it is wanted at some other place and on some other occasion, the heat thus imparted.

We thus have an insight into the operations which are going on in the equatorial belt of precipitation, and this insight is sufficient to enable us to perceive that exquisite indeed are the arrangements which nature has provided for supplying this calm belt with heat, and for pushing the snow line there high up above the clouds, in order that the atmosphere may have room to expand, to rise up, overflow, and course back into the channels of its circulation. As the vapor is condensed and formed into drops of rain, a twofold object is accomplished : coming from the cooler regions of the clouds, the rain drops are cooler than the air and earth below. They descend,

and by absorption take up the heat which has been accumulating in the earth's crust during the dry season, and which cannot now escape by radiation. Thus the equatorial cloud-ring modifies the climate of all places beneath it; overshadowing at different seasons all parallels from 5° S. to 15° N.

In the process of condensation, these rain drops on the other hand have set free a vast quantity of latent heat, which has been gathered up with the vapor from the sea by the trade winds and brought hither. The caloric thus liberated is taken by the air and carried up aloft still further to keep, at the proper distance from the earth, the line of perpetual congelation. Were it possible to trace a thermal curve in the upper regions of the air to represent this line, we should no doubt find it mounting sometimes at the equator, sometimes on this side, and sometimes on that, of it, but so as always to overleap this cloud-ring. This thermal line would not ascend always over the same parallels : it would ascend over those between which this ring happens to be; and the distance of this ring from the equator is regulated according to the seasons.

If we imagine the atmospherical equator to be always where the calm belt is which separates the northeast from the southeast trade winds, then the loop in the thermal curve which should represent the line of perpetual congelation in the air would be always found to stride this equator; and it may be supposed that a thermometer kept sliding on the surface of the earth so as always to be in the middle of this rain belt, would show very nearly the same temperature all the year round; and so too would a barometer the same pressure.

This ring, or band, or belt of clouds, is stretched around our planet to regulate the quantity of precipitation in the rain belt beneath them; to preserve the due quantum of heat on the face of the earth; to adjust the winds; and send out for distribution to the four corners, vapors in proper quantities to make up to each river-basin, climate and season, its due quota of sunshine, cloud and moisture. Like the balance wheel of a well constructed chronometer, this cloud-ring affords the grand atmospherical machine the most exquisitely arranged self-compensation. If the sun fail in his supply of heat to this region, more of its vapors are condensed, and heat is discharged from its latent storehouses in quantities just sufficient to keep the machine in the most perfect compensation. If, on the other hand, too much heat be found to accompany the rays of the sun as

they impinge upon the upper circumference of this belt, then again on that side are the means of self-compensation ready at hand : so much of the cloud surface as may be requisite is then resolved into invisible vessels, in which the surplus heat from the sun is stored away and held in the latent state until it is called for, when instantly these vaporous vessels are condensed, this heat is set free, and becomes a visible and active agent in the grand design.

Returning and taking up the train of contemplation as to the office which this belt of clouds, as it encircles the earth, performs in the system of cosmical arrangements, we may see that the cloud-ring and calm zone which it overshadows is both ventricle and auricle in the immense atmospherical heart, where the heat and the forces which give vitality and power to the system are brought into play; where strength is gathered, and impulse given to the air sufficient to send it thence through its long and tortuous channels of circulation.

That the thermometer stands lower beneath this cloud belt than it does on either side of it, has not been shown, or if shown, it has not yet been made to appear by actual observation, so far as *my* researches are concerned; for the observations in my possession have not yet been discussed concerning the temperature of the air. But that the temperature of the air at the surface under this cloud ring is lower, is a theoretical deduction as susceptible of demonstration as is the rotation of the earth on its axis. It is a well known fact; indeed nature herself has hung a thermometer under this cloud belt that is more perfect than any that man can construct, and its indications are not to be mistaken.

Where do the vapors which form this cloud-ring, and which are here condensed and poured down into the sea as rain, come from? they come from the trade wind regions; under the cloud-ring they rise up; as they rise up they expand, and as they expand they grow cool; moreover, it requires no mercurial instrument of human device to satisfy us that the air which brings the vapor to these clouds cannot take it up and let it down at the same temperature. Precipitation and evaporation are the converse of each other, and the same air cannot precipitate and evaporate, take up and let down water at one and the same time. As the temperature of the air is raised, its capacity for receiving and retaining water in the state of vapor is increased; as the temperature of the air is lessened, its capacity for retaining that moisture is diminished. These are physical laws, and therefore when we see water dripping down from the atmosphere,

we need no instrument to tell us that the elasticity of the vapor so condensed and falling in drops is less than was its elasticity when it was taken up from the surface of the ocean as water, and went up into the clouds as vapor.

Hence we infer that when the vapors of sea water are condensed, the heat which was necessary to sustain them in the vapor state, and which was borrowed from the ocean, is parted with; and that therefore they were subjected in the act of condensation to a lower temperature than they were in the act of evaporation. This is what is going on : ceaseless precipitation, under this cloud-ring. Evaporation under it is supended almost entirely the year round; it is formed by the meeting of the northeast and southeast trade winds. The vapor and the air which they bring with them here ascend, as they ascend they expand, as they expand their temperature falls. Hence, first a cloud and then precipitation. We know that the trade winds encircle the earth; that they blow perpetually and meet each other near the equator; that here the air which they bring ascends, and forms a ring of clouds : by the rainy seasons of the torrid zone, we can trace this cloud-ring like a girdle about the earth.

In view of these facts and of these laws, it is useless to consult the thermometer, merely to learn whether the atmosphere under this cloud-ring be warmer or cooler than that on either side of it. Our knowledge of the laws of nature tells us that it is cooler.

Were the clouds which overhang this belt luminous, and could they be seen by an observer from one of the planets, they would present to him an appearance not unlike the rings of Saturn do to us. Such an observer would remark that this cloud-ring of the earth has a motion contrary to that on its axis of our planet itself; that while the earth was revolving rapidly from west to east, he would observe the cloud-ring to go slowly, but only relatively, from east to west. As the winds which bring the cloud vapor to this region of calms rise up with it, the earth is slipping from under it; and thus the cloud-ring, though really moving from west to east with the earth, goes relatively slower than the earth, and would therefore appear to require a longer time to complete a revolution.

But unlike the rings of Saturn through the telescope, the outer surface of the upper side to us, of this cloud-ring, would appear exceedingly jagged, rough and uneven.

The rays of the sun playing upon this peak, and then upon that, of the upper cloud surface, melt away one set of elevations and

create another set of depressions. The whole stratum is, it may be imagined, in the most turgid state; it is in continued throes when viewed from above : the heat which is liberated from below in the process of condensation, the currents of warm air ascending from the earth, and of cool descending from the sky, all, we may well conceive, tend to keep the upper cloud surface in a perpetual state of agitation, upheaval and depression.

Imagine in such a cloud stratum an electrical discharge to take place, the report being caught up by the cloud ridges above, is passed from peak to peak, and repeated from valley to valley, until the last echo dies away in the mutterings of the distant thunder. How often do we hear the voice of the loud thunder rumbling and rolling away above the cloud surface, like the echo of artillery discharged among the hills.

Hence we perceive or infer that the clouds intercept the progress of sound as well as of light and heat through the atmosphere, and that the contour of this upper surface is often like that of alpine regions.

Again, it is by trains of reasoning like this, that we are continually reminded of the interest which attaches to the observations which the mariner is called on to make. There is no expression uttered by nature which is unworthy of our most attentive consideration; and mariners, by registering in their logs the kind of lightning, whether sheet, forked or streaked; and the kind of thunder, whether rolling, muttering or sharp, may be furnishing facts which will throw much light on the features and character of the clouds in different latitudes and seasons.

As an illustration of the value and interest attached to observations upon "little things" so called, I extract from the Abstract Log of a very close observer who is coöperating with me in the collection of materials for the "wind and current charts." "In all my observations," writes this excellent and indefatigable seaman, in his Abstract Log kept for me; "in all my observations on the tints of tropical flowers, I have found that yellow predominates."

No physical fact is too bald for observation; physical facts are the language of nature, and every expression uttered by her is worthy of our most attentive consideration. And the remark by this observant sailor about the predominance of yellow in tropical flowers, would, as a truism, be regarded with a high degree of interest both by the botanist and the chemist.

Navigators are now learning to tell by the barometer when they have passed from under this cloud-ring. By a log received the day before I left Washington, I was struck with the master's remarks upon the subject. Capt. John H. Young, of the ship Venice of Philadelphia, on a voyage from New-York round the world in 1850–51, observes :

"I here predict," says this clever navigator, just before reaching the equator in the Atlantic ocean on his outward passage — "I here predict the barometer will remain below 30^{in}, until we get without the influence of the rainy latitudes."

After having crossed a belt of 5° or 6° of latitude in breadth, within which such remarks as these are frequent : 'Weather warm and sultry;' 'heavy rain;' 'very murky and close at times;' 'quite oppressive;' 'rain," etc.

On the 7th day, however, he remarks : "Assuming the settled weather of the 'trades,' only requiring a rise in the barometer to assure me of that fact."

The day after, I find in his column of remarks : "Fine weather; every appearance of trades; barometer up." This remark is made March 5th, 1850, in 6° south. Had he passed that way in this month of August, he would probably have made it in 6° north; for his barometer then would have been 'up,' indicating that he had passed from under the influences of the cloud-ring.

Thus we arrive at a new application of the barometer; for by carefully observing it, the navigator may tell, where other means fail him, when he enters and when he leaves the trade-wind region.

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36. On the Progress of the System of Meteorological Observations conducted by the Smithsonian Institution, and the propriety of its immediate extension throughout the American Continent. By Prof. Arnold Guyot, of Cambridge.

Prof. Guyot first showed the importance of these observations to the thorough knowledge of meteorology, and circulated plates and sheets prepared to direct observers as to the classification of the clouds, and giving the form in which the observations, and indicating the times and manner in which the notations ought to be made. He exhibited also the instruments provided by the Association, such as

psychrometers, thermometers, etc. Printed tables were also exhibited, which exemplified how the Association had published the various mean results which had been obtained in one place. For instance, the published table exhibited that at North-Salem in Westchester county, in the month of June, each day there was taken three times, at the hours of 6 A. M., 2 P. M. and 10 P. M., observations of the meteorologic state of the atmosphere, as follows : The phase of the moon, the barometrical indication, the height of the thermometer, direction and force of the wind, the plants in flower, the migratory birds first seen, the state of the psychrometer, the force of vapor, humidity, the state of the rain-gage, the state of cloudiness, with notes of the various kinds of clouds visible.

Prof. GUYOT stated that there were but fifty places of observation as yet established, and he exhibited how very small a portion of this continent had as yet been covered by those fifty stations. He pointed to the vast table land which reached from the Mississippi to the Rocky Mountains. This vast table land, he believed, exerted more influence on the meteorologv of the continent than even the Rocky Mountains. He then pointed, by colored sections, to the various positions at which observations ought to be taken : these various lines extended from the mouth of the Columbia to the St. Lawrence, from San Francisco to Washington, from the Gulf of California to the Rio del Norte, from the Pacific across the plateau of Mexico to the West India Islands, across the isthmus of Tehuantepec, at Nicaragua lake, at the plateau of Costarica, and at the strait of Panama to Chagres.

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37. ON THE METEOROLOGICAL OBSERVATIONS OF NEW-YORK FROM 1825 TO 1850. By FRANKLIN B. HOUGH, A. M., M. D., of Somerville, N. Y.

IT is well known that in 1825 the Regents of the University of New-York, following the example of the Federal Government in its instructions to the commandants of its various military posts, issued orders to the several academies subject to their visitation, requiring them to cause meteorological observations to be made after a specified form, and with instruments furnished them for the purpose. These observations, which at that time were supposed to fulfil all

the requirements of science, embraced three daily records of the thermometer, with the direction of the wind, and appearance of the sky as clear or cloudy, in the forenoon and afternoon; a record of the rain-gage; and such observations on storms, meteors, auroras, and the progress of vegetation, as might be deemed worthy of note.

This system of observations was maintained with more or less fidelity and regularity, during twenty-four years, by sixty-two literary institutions of New-York, the aggregate period of which amounts to eight hundred and thirty-four years and eight months; and the results, published annually in the reports of the Regents, have been acknowledged both in this country and Europe as valuable contributions to the science of meteorology.

The progress of this science having required additional and more accurate observations with improved instruments, the system of 1825 was discontinued at the close of 1849, to give place for the present very thorough and efficient course of observations.

Although these records are without value in determining the extent and progress of storms, and the various atmospheric vicissitudes which are indicated by the delicate instruments now in use; yet they are invaluable in establishing the laws of climate, the mean temperature, depth of rain, and general character of the weather of the several years and at the different stations.

Impressed with the great value and importance to science of these records, and aware of the difficulty that must attend reference to them, scattered as they are through our legislative documents for a quarter of a century, the writer, several months since, undertook the labor of reducing the entire series, with the view of obtaining therefrom every fact that might possess any value, or elucidate any subject in relation to the meteorology of the State.

It is intended to offer the results of these, when completed, to the legislature of New-York, for publication; and with the hope of insuring a favorable reception with that body, the Association is respectfully solicited to refer this subject to the meteorological committee, to examine the labor already performed, and the details of the plan intended to be followed; with the view of enabling them to express an opinion of its merits, and of recommending it, if found worthy, to the favorable notice of the New-York legislature.

38. On the Maxima and Minima of Temperature at Hartford, Conn. By Prof. J. Brocklesby, of Trinity College, Hartford.

In the spring of the year 1847, among other meteorological observations, I commenced recording the daily maxima and minima of temperature, in order to ascertain several particulars in respect to the thermal fluctuations of the atmosphere in the locality where I resided. The instrument employed for observing the range of temperature was a Six's thermometer of nice construction, made by Cary, of London. It was enclosed in a box, perforated with holes in such a manner as to admit of a free circulation of air around the instrument when the door of the box was closed.

The box was placed on the north side of a building, six feet above the ground, and so attached to the edifice that an open space was left for the passage of air between the back of the box and the building. Thus situated and enclosed, the instrument was shielded from the direct rays of the sun, as well as from the heat radiating from contiguous objects; and correct indications of temperature were secured, so far as they were dependent upon avoiding those causes of error that are connected with location.

For the space of twenty-one months, the daily maxima and minima were recorded, with the loss from unavoidable circumstances of only a very few observations during this period. The series of observations would have been continued, had not a sudden disarrangement of the mercurial column of the thermometer occurred at the end of this time, which rendered the instrument useless.

From the data thus obtained, I have constructed the accompanying map*, which exhibits at a glance the maxima and minima of temperature throughout the period referred to. In the system of ruled lines, the ordinates represent degrees and fractions of a degree of temperature, and the abscissas (running the whole length of the map) the divisions of time; either one of the horizontal sides of each of the smallest squares indicating a *day*, and the adjacent side a *degree*. The course of the maxima follows the lower margin of the red line, while that of the minima is traced by the upper boundary of the blue band. The distance between the red and blue bands in degrees, at any particular day, gives the range of temperature for that day as indicated by the maximum and minimum.

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* The map has been omitted on account of its size.

The sum of the daily ranges for the period of observation, extending through six hundred and forty-three days, is nine thousand four hundred and forty-eight and forty-two hundredths degrees (9448,42°), and the average diurnal fluctuation is 14,694°.

The daily average variation for one year, beginning on the first of January 1848, and closing the first of January 1849, was found to be 14,668°; differing but (0,026°) twenty-six thousandths of a degree from the daily average fluctuation of the entire period.

The sum of the thermal oscillations for each month, the number of the days of observation, and the mean daily variation for each month in the year, are given in the table below.

Months.	1847.	1848.	1849.	Days of observation.	Sum.	Mean daily variation.
January...	....	392,15°	328,74°	55	720,89°	13,107°
February..	....	379,13	....	29	379,13	13,07
March	....	445,80	....	30	445,80	14,86
April	269,82°	606,75	....	42	876,57	20,87
May	596,19	446,50	....	62	1042,69	16,82
June......	431,98	476,35	....	60	908,33	15,139
July	508,18	390,60	....	62	898,78	14,497
August....	432,96	489,82	....	62	922,78	14,883
September.	358,19	485,97	....	60	844,16	14,07
October ..	534,90	462,87	....	62	997,77	16,093
November.	371,80	388,13	....	57	759,93	13,33
December .	305,85	345,84	....	62	651,69	10,51

From this table, it appears that the several months rank as follows in respect to their variability of temperature:

1. April.
2. May.
3. October.
4. June.
5. July.
6. August.
7. March.
8. September.
9. November.
10. January.
11. February.
12. December.

The first seven months of this list possess an average daily range above that of the entire year, and the remainder are more or less below.

The mean thermal oscillation for April exceeds by more than six degrees that of the year, while December ranges a little over four degrees below.

By dividing the year into the four astronomical portions marked

by the solstices and equinoxes, and finding the mean of the oscillations of temperature belonging to each division, we obtain the following results for the year beginning the 22d of December 1847, and ending the 22d of December 1848 :

From the winter solstice to the vernal equinox, the mean is	12,828° ;
From the vernal equinox to the summer solstice, ...	16,91 ;
From the summer solstice to the autumnal equinox, ...	14,66 ;
From the autumnal equinox to the winter solstice, ...	13,237.

Throughout the range of the year, the average fluctuation of temperature is therefore *greatest* in the spring, *least* in the winter, and more in the summer than in the fall.

The most extensive diurnal variation within the period of time under review is thirty-seven degrees and a half, and occurred on the 24th of December 1848. On the 9th and 10th of April of the same year, the oscillations of temperature were respectively thirty-five degrees and thirty-three and a third degrees; and in three other instances only the range of the daily temperature exceeded thirty degrees. The lowest daily variation took place on the 25th of September 1848, when the difference between the maximum and minimum amounted only to one twentieth part of a degree*.

Although the preceding results show that the locality where the observations were taken partakes largely of the proverbial fickleness of the climate of New-England, nevertheless the sum of 9448,42° does not indicate, as will readily be seen, all the thermal fluctuations embraced within the period of time whence we derive this sum : neither does 14,668° express the full annual mean of the variations. In order to obtain these elements with perfect exactness, we should not confine our observations to the daily maximum and minimum ; but all the oscillations that occur during any period of time should either be marked and recorded by an unbroken series of observers, or by a self-registering instrument, and the true sum and mean of the thermal variations can then be readily obtained. Just as in order to ascertain the contour of a rolling and rising tract of country between two assumed stations, we should measure the extent of the successive elevations and depressions, and not content ourselves with

* The highest temperature during the period of observation occurred on the 17th of June 1848, when the thermometer stood at 90,70° ; and the lowest took place on the 12th of January 1849, the column standing at 6,87° below zero.

simply determining the difference in the height of the two stations. For this reason, the preceding results are to be regarded as the lowest numerical expressions for the elements they respectively represent.

I cannot, in this connection, refrain from remarking that the subject of the range of variation in atmospheric temperature seems as yet not to have received that attention which it deserves. I might speak of its importance in several respects, but will allude only to one particular.

Within a few years, the medical faculty have been led to investigate the connection between human life and health, and certain conditions of the atmosphere; and Prof. Caspar of Berlin has shown, from a most extensive research involving immense labor, that the life and health of man is intimately affected by the *temperature*, *elasticity*, and *hygrometric state* of the atmosphere. He finds that there is no condition of the atmosphere which influences health so much as temperature; and that extremes of temperature, whether *high* or *low*, are eminently destructive of life. Are we then to infer that *every* thermal fluctuation above or below the mean weakens the silver cord of life; or that these changes of temperature strengthen and invigorate the frame when of moderate extent, but become injurious only in their excess? If the latter view is correct, what is the limit? At what point do these oscillations cease to be beneficial and become injurious; and does this point (if it exists) vary with respect to age, sex, and condition? To answer these and other kindred questions equally interesting and important, opens a vast field of labor, which I trust will not be neglected.

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39. On the Quantity of Rain at different Heights, from Observations made at the Institution for the Deaf and Dumb, New-York City. By Prof. O. W. Morris.

A few years since, some notes on the quantity of rain that fell during a period of sixteen months, were published, with a hope that other observers in this country would make observations to illustrate the same subject, and aid in arriving at some satisfactory conclusion on it. I have waited in vain for something of the kind from my fellow-

laborers in meteorology, and now give the results of further observations, without, however, offering any conclusions; leaving it for some more favorable time and more competent person, when an extended series, embracing a great variety of climate and attendant circumstances, may afford better data than are within reach at the present time.

The time embraced in the present series is from January 1, 1846, to December 31, 1850, a period of five years. There are, however, a few deficiencies for the months of July and August in the years 1847 and 1848, that render the true average of these months imperfect.

The results are arranged as follows:

I. The quantity, difference and average for each month, and per year.
II. do do do for each season.
III. do do do for the six warmest and the six coldest months.
IV. do do do for the warmest & the coldest month.
V. The greatest and the least quantities in one month.
VI. The greatest and the least differences in one month.
VII. The particular year and month in which the greatest quantity fell in the five years.
VIII. The year and month in which the least quantity fell.

I. Month.	Upper Gage.	Lower Gage.	Difference.	Average.
January	14,145	17,731	3,586	,717
February	21,350	24,460	2,110	,522
March	18,145	22,635	4,490	,898
April	6,000	8,025	2,025	,405
May	21,562	26,029	4,467	,893
June	18,005	23,835	5,830	1,160
July	15,190	19,240	4,050	,810
August	10,720	12,770	2,050	,410
September	15,840	18,930	3,090	,618
October	15,835	19,440	3,605	,721
November	13,122	20,345	7,233	1,466
December	17,220	22,460	5,240	1,048
Total	187,134	234,900	47,776	9,668
Average per year,	37,426+	46,980	9,553+	,808
do month,	3,118+	3,915	,796+	
II. Spring	45,707	56,689	10,982	3,666
Summer	43,915	55,845	11,930	3,976
Autumn	38,237	51,110	12,873	4,291
Winter	52,715	63,651	10,936	3,645
III. Six warmest	87,317	108,829	21,512	3,585
Six coldest	93,257	118,466	25,209	4,201

	Upper Gage.	Lower Gage.	Difference.
IV. Warmest month..	15,190	19,240	4,050 in July.
Coldest month ...	14,145	17,731	3,586 in January.
Difference	....	....	,464
V. Greatest quantity,	18,005	23,835	5,830 in June.
Least quantity ..	6,000	8,025	2,025 in April.
Difference	....	....	3,805
VI. Greatest difference, 7,223 in November.			
Least do 2,025 in April.			
Difference.... 5,198.			
VII. Greatest quantity in one month, 9,750 in May 1846.			
Greatest difference do 2,025 in November 1846.			
VIII. Least quantity in one month, 0,650 in January 1849.			
Least difference do 0,040 in October 1850.			

The gages are the conical gages formerly used by the academies in this State. The lower one is five feet above the surface of the ground; the upper, seventy-one feet above, on the roof of the institution.

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40. On Ocean Temperatures. By Lieut. M. F. Maury, Superintendent U. S. National Observatory.

[*Not received.*]

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41. A Comparison of the Apparent Diurnal Laws of the *Irregular* Fluctuations of the Magnetical Elements, at the Stations of Observation in North America. By Capt. J. H. Lefroy, Royal Artillery, F. R. S., Director of H. M. Magnetical Observatory at Toronto, Canada.

One of the principal objects proposed by Baron Alex. Humboldt, in the great scheme of concerted magnetical observation originated by him in 1828, was to examine those "eccentricities of the needle, of

" which a certain periodicity had been affirmed by M. Kupffer, and " which appeared to Baron Humboldt to be the effect of a reaction " from the interior towards the surface of the globe; as he ventured " to say, of *magnetic storms*, indicating a rapid change of tension*." He referred particularly to oscillations which were frequently repeated at the same hours before sunrise. In the great extension of that scheme of observation which resulted from his memorable letter to H. R. H. the Duke of Sussex in 1836, although the recent discoveries connected with the irregular variations of the magnetic declination are referred to† as giving to that class of changes a prominent interest, their periodicity does not appear to have been particularly kept in view: on the contrary, they are classed as " the " *irregular* variations, or those which *apparently* observe no law." The distinguished Superintendent of the British Colonial Observatories, however, at once recognized the importance of making a distinction between the effects of the two distinct and probably very different influences which, under the denominations of *regular* and *irregular*, are combined in the diurnal and annual changes actually presented by observation. In Col. Sabine's first official publication, the "Observations on days of unusual disturbance" (part 1, 1843), he examined the observations of the year 1841 at Toronto and Hobarton, by a method of his own, for the purpose of assigning *precisely* the relative degree to which the elements of declination and horizontal force are subject to disturbance at each hour of the twenty-four. As regards the latter, the observations of that particular year furnished no very definite result. As regards the declination, however, the observations at Toronto and Hobarton agreed in showing that its mean *irregular* fluctuation is considerably greater during the night than during the day, and has a well marked epoch of maximum value, which was found to be from 8^h to 10^h P. M. at Toronto, and from 9^h to 13^h P. M. at Hobarton (the even Göttingen hours being taken at both stations, the intervals correspond to different periods of mean time). The former of these is the period for which M. Kreil had also found the greatest liability to disturbance at Milan and Prague (1838 – 40)‡; and both Dr. Lloyd and Mr.

* Proceedings of the Royal Society, Vol. iii, p. 418.

† Report of the Joint Committee of Physics and Meteorology, made to the Council of the Royal Society, 1839.

‡ Letters to M. Kupffer and Col. Sabine. Phil. Mag. 1840, pp. 241 – 418.

Broun have arrived at very similar results, from their discussion of the observations at Dublin and Makerstown respectively. We are therefore warranted in assuming as the result of all the observations discussed heretofore, in reference to this class of influences, first, that the declination has only one principal epoch or maximum of mean disturbance; secondly, that this occurs everywhere at about 10 P.M. of local mean time. The object of this paper is to point out that the diurnal law thus stated undergoes a remarkable modification on this continent, in more northern latitudes; and that in any physical explanation of the phenomenon, it will be necessary to account for two instead of only one diurnal period of maximum mean disturbance.

Various methods have been practised of eliminating from the first results of observation, that part of the changes which is to be regarded as a function of the solar time, and classed as *regular;* as well as of determining the mean value of the remaining or irregular effects. In the investigation of Col. Sabine above referred to, the differences were first taken between the arithmetical mean of all the observations under each hour, and each of those observations individually; the difference between the value of the remainders for two successive observation hours, was regarded as the *irregular fluctuation* between those hours; and the square root of the mean of the squares of these second differences, for each interval, is what is called the *mean irregular fluctuation* for that interval. The present results have been obtained by the less laborious method adopted by Dr. Lloyd, which consists in squaring the first differences ($\Delta\pm$), and regarding the square root of the mean of these squares $\left(\sqrt{\frac{\Sigma(\Delta^2)}{N}}\right)$ as the *mean disturbance* of the elements for the hour, in analogy with what are called *mean errors* of observation.

The following table contains the value of the mean disturbance of the declination for each hour of the day and night, at Toronto, Sitka, and Fort Chipewyan on Lake Athabasca, in Lat. 58° 43′, Long. from Greenwich 7^h $25{,}2^m$ W. The period of observation compared is the winter of 1843 – 4, from October to February inclusive; and as the observations were made at the hours of Göttingen mean time at all the stations, they were practically simultaneous.

TABLE I.

Mean disturbance of the declination, October – February.

Local mean time.	Toronto.	Sitka.	Lake Athabasca.	Local mean time.	Toronto.	Sitka.	Lake Athabasca
Midnight	1',72	4',36	8',5	Noon	1',61	2',67	4',6
13	1,38	4,18	9,9	1	1,54	2,55	4,3
14	1,60	3,62	8,6	2	1,34	2,20	4,7
15	1,59	2,97	7,4	3	1,71	2,14	4,7
16	1,52	2,92	11,0	4	1,81	1,95	4,4
17	1,67	2,84	15,2*	5	1,86	1,81	3,7
18	1,75	2,75	10,6	6	1,02	1,90	4,3
19	1,29	2,82	8,5	7	0,84	3,09	3,8
20	1,82	2,88	5,5	8	1,53	2,47	4,6
21	1,89	2,80	4,6	9	2,16	2,51	6,3
22	1,52	3,10	5,1	10	2,72	3,43	10,2
23	1,68	2,79	7,0†	11	2,27	3,32	9,7

In the next table, the same two stations are compared with Fort Simpson, on M'Kenzie's river, in latitude 61° 52′, long. 8^h 5^m W. The period of observation compared is here the months of April and May, 1844; embracing, however, only 46 observation days at the most northern station, but the full number at the others. Sundays are included at Sitka in both cases.

TABLE II.

Mean disturbance of declination, April – May 1844.

Local mean time.	Toronto.	Sitka.	Fort Simpson.	Local mean time.	Toronto.	Sitka.	Fort Simpson.
Midnight	2',89	5',18	14',5	Noon	2',05	2',58	8',0
13	3,16	6,57	11,7	1	1,84	2,22	4,6
14	3,83	5,42	15,8	2	1,71	2,31	7,0
15	4,62	8,01	22,2	3	1,56	2,81	6,3
16	3,77	8,92	17,3	4	1,68	2,36	6,2
17	2,42	5,08	25,6	5	1,88	2,96	7,4
18	2,43	3,60	22,9	6	2,04	4,03	8,1
19	3,97	2,69	22,3	7	2,78	4,16	8,8
20	2,91	2,49	19,7	8	2,29	2,91	12,3
21	2,16	2,10	12,0	9	2,94	3,51	15,4
22	2,08	2,46	12,2	10	4,58	3,90	13,1
23	2,24	2,57	6,2	11	2,73	3,58	9,2

* By omitting one observation on January 25^d 1^h Gött., when the extreme point of a great movement happened to coincide with the regular hour of observation, this value is reduced to 10,6′.

† By omitting a similar extreme observation on February 2^d 7^h Gött., this value is reduced to 6,7′.

The observations at Toronto were taken $2\frac{1}{2}^m$ after the hours named; those at Sitka, 28^m after; those at Lake Atteabasca, 5^m after; and those at Fort Simpson, 15^m before the hours named.

It is not to be expected that the observations of periods so short as five months and two months, should exhibit the diurnal law of mean disturbance with much regularity, unless at stations peculiarly liable to that effect. Lake Athabasca and Fort Simpson are such stations, and here the law is strongly marked : the corresponding periods at the other stations are introduced for strict comparison; but we must take a longer period, as in the next table, to which I have added Philadelphia*, to make their characteristics in this respect fully evident.

TABLE III.

Mean disturbance of the declination for twelve months at Philadelphia and Sitka, and for two years at Toronto; to which are added for comparison, the corresponding values given by Dr. Lloyd for the year 1848, from the observations at Dublin†.

Local mean time.	Dublin.	Philadel.	Toronto.		Sitka.
			1843.	1844.	
Midnight	....	2',22	1',78	3',03	4',57
13	2',81	2,10	2,06	2,72	4,95
14	....	2,29	1,91	2,62	4,07
15	2,52	2,29	1,92	2,89	4,35
16	....	1,91	1,83	2,85	4,93
17	2,16	2,10	2,17	2,72	3,51
18	....	2,06	2,25	2,53	3,03
19	1,93	2,20	1,82	3,08	2,86
20	....	2,00	1,67	2,28	2,91
21	1,89	2,19	1,98	2,07	2,65
22	....	1,91	1,91	2,08	3,14
23	1,93	1,93	1,85	2,14	2,83
Noon	....	1,81	1,93	1,88	2,60
1	2,17	1,65	1,83	1,80	2,40
2	....	1,76	1,66	1,92	2,22
3	2,11	1,77	1,70	1,83	2,38
4	....	1,74	1,83	1,76	2,45
5	2,12	1,67	1,73	1,96	2,52
6	....	1,62	2,05	1,87	2,71
7	2,44	1,86	1,87	1,95	3,32
8	....	2,52	2,56	3,35	4,50
9	3,47	2,61	2,43	3,56	4,05
10	....	2,59	2,44	3,38	4,42
11	4,07	1,88	2,32	2,88	3,67

* Magnetical and meteorological observations at Girard College, by Dr. A. D. Bache, 1840 – 1845.

† Transactions of the Royal Irish Academy, Vol. xxii, part 1.

The year 1843 was selected by Dr. Lloyd for this investigation, on account of its freedom from great disturbances : "on the grounds "that the number which denotes the frequency of the irregular "changes, in consequence, bearing a larger proportion to that which "denotes their magnitude, any regular law to which they are subject "will be more readily apparent." The observations of 1843 at Toronto, as compared with those of the following year, show that this absence of disturbance was equally characteristic at that station. The observations at Philadelphia and Sitka belong principally to the year 1844, the period examined being the twelve months commencing October 1843.

It appears by the foregoing table, that at each of the American stations the mean disturbance of the declination has its lowest value in the afternoon, and that this is succeeded by a maximum at 9 or 10 P. M. So far, therefore, the diurnal law at all the stations agrees with that deduced by Dr. Lloyd from his own observations, namely, that "during the day, i. e. from 6 A. M. to 6 P. M., the mean disturbance is nearly constant : at 6 P. M. it begins to increase, and arrives at a maximum a little after 10 P. M." When, however, Dr. Lloyd proceeds to state, "it then decreases with the same regularity, and arrives at its constant day value at about 6 A. M.," he describes a feature which is evidently *not* characteristic of all the stations, and is more completely wanting as we proceed to the north.

The following are the arithmetical means of the values for each quadrant of the twenty-four hours :

TABLE IV.

	Midn. to 5 a. m.	6 to 11 a. m.	Noon to 5 p. m.	6 p. m. to midn.
Dublin.........	2',50	1',92	2',14	3',32
Philadelphia ...	2,15	2,05	1,73	2,18
Toronto { 1843.	2,11	1,91	1,78	2,28
Toronto { 1844.	2,80	2,36	1,86	2,83
Sitka	4,40	2,90	2,43	3,86

It will be seen that at Philadelphia and Toronto, the mean value from midnight to 5 A. M. is somewhat less than in the quadrant preceding midnight; but the difference is materially less in proportion than at Dublin : at Sitka, the highest value is after midnight. Arranging in like manner the values for the shorter periods in Tables I and II, we find the same general law, but with a greater excess after midnight at Lake Athabasca than at Sitka, and a yet greater at Fort Simpson than at Lake Athabasca.

TABLE V.

Mean disturbance of declination, October 1843 to February 1844.

	Midn. to 5 a. m.	6 to 11 a. m.	Noon to 5 p. m.	6 p. m. to midn.
Toronto........	1',57	1',66	1',64	1',76
Sitka...........	3,48	2,86	2,22	2,79
Lake Athabasca,	9,33	6,88	4,40	6,48

TABLE VI.

Mean disturbance of declination, April and May 1844.

	Midn. to 5 a. m.	6 to 11 a. m.	Noon to 5 p. m.	6 p. m. to midn.
Toronto	3',45	2',63	1',79	2',89
Sitka..........	6,53	2,65	2,54	3,68
Fort Simpson ...	17,85	15,88	,568	11,15

The observations of April and May at Sitka include nine days which were not days of observation at Toronto, and fourteen which were not days of observation at Fort Simpson, where there were also a few omissions in the sixhourly period commencing at midnight: this may account for the somewhat lower relative value in that quadrant as compared with the one preceding it, at the latter station, than at Sitka.

The above comparison exhibits a marked difference in the state of disturbance prevailing after midnight at Sitka, Lake Athabasca and Fort Simpson, as compared with the lower stations. Of the six hours preceding midnight, however, only 9^h, 10^h and 11^h are distinguished for a high value of this quantity at any of the stations: it will perhaps be better therefore to compare these three hours with 3^h, 4^h and 5^h A. M. particularly; hours to which no prominent interest was assigned by any of the authorities quoted above. I have here taken, in each instance except Dublin, the value of $\sqrt{\frac{\Sigma(\Delta^2)}{N}}$: it is also necessary to substitute at Dublin some of the values of 1843 for the corresponding months of 1844; but this will not affect the comparison. Taking the same periods at all the stations, we have the following results:

TABLE VII.

	FIVE WINTER MONTHS.		TWO SPRING MONTHS.	
	9 - 11 p.m.	3 - 5 a.m.	9 - 11 p.m.	3 - 5 a.m.
Dublin.........	2',70	1',75	5',27	2',71
Philadelphia ...	1,85	1,47	2,17	2,46
Toronto	2,35	1,59	3,51	4,17
Sitka	3,10	2,80	3,43	7,50
Lake Athabasca,	8,89	11,63		
Fort Simpson ...	....	....	12,89	21,57

Again : for the whole year, we have

TABLE VIII.

	9 - 11 p.m.	3 - 5 a.m.	
Dublin.........	3',77	2',34	As 1,61 : 1
Philadelphia....	2,38	2,11	1,13 :1
Toronto 1843.	2,39	1,98	1,21 : 1
Toronto 1844.	3,13	2,82	1,11 : 1
Sitka	4,19	4,28	0,97 : 1

The highest values would be given at Sitka by the hours 12^h, 1^h, 2^h A. M. both in the winter period and on the whole year, and by 2^h, 3^h, 4^h A. M. in the two spring months, which appears to show an earlier epoch for the second reaction than at the other two northern stations; but probably the reduction of another year's observations, at least, will be necessary to determine this point.

It appears then that while at Philadelphia and Toronto, as at Dublin, the mean disturbance of the declination presents but one maximum, which occurs at 9 or 10 P. M.; yet there is not, even at these stations, a complete agreement. At Dublin, the value decreases regularly after 11 P. M.; at Philadelphia and Toronto, it decreases to a materially less degree. Proceeding to Sitka, we find a maximum about the same hour, but it is succeeded by another at 1 A. M., and the average value is somewhat greater for the hours succeeding than for those preceding midnight. At Lake Athabasca, we have still a maximum about 10 P. M., but it is decidedly inferior to a second maximum at 5 A. M. or thereabouts. Lastly, at Fort Simpson, without losing the first maximum, we find it exceeded in a still higher ratio by the second. Hence we are led to the conclusion that, as regards the declination, there are two classes of irregular influences, or two reactions during the night, succeeding the regular influences which have acted during the day. Of these, the one which produces a ma-

ximum value of disturbance at 9 or 10 P. M. appears to be universal, but is nowhere important enough to modify in any marked degree the character of the mean diurnal curves of this element, which is chiefly impressed, at all save the most northern stations, by the direct or regular action of the day. The other class, or that which produces a second maximum of disturbance after midnight, is not universal, but comes into operation, on the American continent, more and more effectively as we approach the magnetic pole; until at last, as will be seen by the accompanying diagrams, its energy is such as to mask the effect of the more feeble regular influences, and to determine almost entirely the apparent character of the mean diurnal changes.

I have hitherto referred to the disturbance of the declination alone, that being the element which has been most generally studied, and which alone has given a marked maximum of disturbance at 9^h or 10^h P. M. The horizontal force and the inclination, at Lake Athabasca and Fort Simpson, equally support the conclusion that the causes producing disturbance at these stations come into operation chiefly towards morning, and concur in showing 4^h or 5^h A. M. to be the period of their greatest effect. The result is that the principal inflexion in the mean diurnal curve for each element occurs at that hour also, giving to these curves an apparent character differing most remarkably from those described elsewhere.

In the accompanying plate, I have laid down the simultaneous mean diurnal curves for the three elements at Lake Athabasca and Toronto, for the five months October 1843 – February 1844 : it will be seen that so completely are the regular changes at the northern stations subordinate to the irregular, that the effect of the former is not easily recognized at all. By omitting all the days on which disturbances were observed, amounting to about three fifths of the whole number, it was, however, found that a considerably greater degree of correspondence could be distinguished; the great inflections at 3, 4, 5 A. M. were materially reduced, and a greater prominence given to the inflections at midday. Hence it appears probable that if we could eliminate the effects of the disturbances, or of the influences classed as *irregular*, entirely, there would be little other difference in the remaining curves, than must be expected to result from the great difference in the length of the day and night at these two stations.

The two months observations at Fort Simpson give remarkably

similar curves to those at Lake Athabasca, and even exaggerate their peculiar features, namely, the inflections at 3 – 5 A. M., and the singular prolongation of a high value of the horizontal force to a late hour.

It can scarcely be necessary to point out the great comparative amount of the mean disturbance shown at the two most northern stations. At Fort Simpson this may be in some slight degree artificial, the result of a change in the mean scale readings; but such cannot be the case to any material extent, as steps were taken to eliminate that change before this calculation, as is shown at large in the account of the observations at these stations. The effect of this greater prevalence of disturbance is strikingly shown in the great amount of the mean daily range of the elements. Taking the difference between the highest and lowest scale reading of each day for the daily range, and $\sqrt{\frac{\Sigma(\Delta^2)}{N}}$ for the *mean* daily range, we have the following remarkable series:

TABLE IX.

	FIVE WINTER MONTHS.		TWO SPRING MONTHS.	
	Declination.	Horizontal force.	Declination.	Horizontal force.
At Philadelphia,	7',1	,00132x	11',4	,00157x
Toronto	8,5	,00242x	14,0	,00357x
Sitka	9,8	,00380x	16,2	,00444x
Lake Athabasca,	30,4	,02768x		
Fort Simpson ...	....	....	63,1	,04182x

Bearing in mind that Sitka, which differs so widely from Fort Simpson and agrees so nearly with Toronto, is 460 geographical miles distant from the former and 2250 from the latter*, we have here a striking proof how little magnetical phenomena are governed by geographical relations.

There is one other circumstance connected with the disturbance of the declination, which I cannot forbear to mention, although it

* Sitka: Latitude 57° 3'; longitude 9^h $2,2^m$ W. from Greenwich. It may be necessary to mention that the unifilar magnets at Philadelphia and Sitka were of 2 feet in length; at Toronto, of 14 inches; and at the two northern stations, of 3 inches only. The effect of the dimensions of magnets on the amount of their movements, and upon the mean diurnal curves deduced from those movements, is a subject which requires further investigation.

does not immediately belong to the subject of this paper. I mean the remarkable influence of the seasons upon the mean diurnal curve of this quantity; the great augmentation which the principal maximum receives at the time of the equinoxes, and the much lower values which prevail at both the solstices. In the next table are shown the values of the mean disturbance of declination at Philadelphia, Sitka and Toronto, for one year, according the astronomical seasons*. Thus February, March, April, form the group for the vernal equinox, and so on : each quantity is the value of $\sqrt{\frac{\Sigma(\Delta^2)}{N}}$; the whole reduced to arc. (See Table X.)

Although it would be improper to unite in one mean seasons so dissimilar as midwinter and midsummer, it will be remarked that they differ much less than might have been expected; and that the difference in the epoch of greatest mean disturbance is very small, in proportion to the difference in the length of the day at these seasons. The means for the year have been already given.

It seems natural to connect this remarkable prevalence of disturbance at the equinoxes with the well known fact that the aurora borealis is most developed at the same seasons. I do not mean to offer one fact as explaining or accounting directly for the other; but believing the latter to be an entirely atmospheric phenomenon, subject to periodic laws, both diurnal and annual, to suggest that both may be related to a common cause. The magnetical phenomena seem to show that there are two classes of forces, characterized by determining the equatorial end of a magnet to the east and west respectively, and that these are severally brought into operation by the presence and absence of the sun above the horizon : when these forces are nearly balanced, owing to the equal length of day and night, or his position near the equator, then disturbances prevail; when either of them greatly preponderates, as happens alternately in winter and summer, there is a less disposition to disturbance. As regards the elements of magnetism, the whole diurnal change then derives its peculiar character principally from the forces proper to the day or night, as the case may be : as regards the aurora, con-

* This arrangement was adopted after actual trial of the more usual division according to meteorological seasons, which did not exhibit the characteristic sought, in such strong contrast.

sidered as a visible electric discharge, it would seem an inference that the causes producing it are diminished by the same circumstance.

In the foregoing tables, the differences have been summed without regard to their direction or sign. If we sum separately the squares of the differences which have the + and — signs, or which indicate deviations to the east and west of the supposed mean position for the hour, it is found that at every season, and at each of the stations, the maximum of mean disturbance at 9^h or 10^h P. M. is the result of easterly movements. Such is also the case with the maximum at 5 A. M. at the two most northern stations. The westerly means, on the contrary, are the largest during the day; and there are indications of a maximum value under this sign at 6 or 7 A. M., but less regular, and apparently more affected by the seasons than the other.

EXPLANATION OF THE PLATE.

The mean diurnal curve of declination at both stations for the five months compared, is laid down on a scale of 10,0′ to one inch : that of the horizontal force at Lake Athabasca, upon a scale of ,00341+ to one inch; the same at Toronto, on a scale twice as large, or ,00171 to an inch.

The curve of inclination at Lake Athabasca, from the scale readings of the induction inclinometer, is on a scale of 0,58′ to an inch, being in proportion to that of the horizontal force. The corresponding curve at Toronto is on a scale of 0,151′ to an inch, which is also in proportion to that of the horizontal force at the same station; but the values laid down are from the mean scale readings (for the same period) for *three years* : consequently not directly comparable, as regards their amount, with those of the horizontal force, which exhibit a considerably less than average diurnal change, in consequence of the general absence of disturbance in the winter of 1843 – 4.

Dr. Bache said that the Section were deeply indebted to Captain Lefroy for the very beautiful illustration of his observations. When he said this was but a leaf from his "log book," the members must have some idea of the immense labor of which this was an example of only one day's work. These observations were of the highest importance to every investigator of magnetical phenomena, and must have a most beneficial effect on the researches which were going on among scientific men in almost every quarter of the globe to ascertain the true laws of magnetism.

Com. Wilkes had witnessed the beautiful exposition of Captain Lefroy with sentiments of high admiration; and he would take this opportunity of calling the attention of the Section to the propriety

of directing measures to be taken to lay before Congress the necessity of establishing stations for a series of observations of a like character. This was an investigation that could not be pursued by private or individual exertions. As the benefits which would result from it would accrue, not to the individuals who might take it up, but to the whole country and especially to our marine, it was but just that Congress should aid in affording the means for performing the labor attached to such investigation in a worthy manner. The British Government had set a worthy example : one which we ought to emulate.

Prof. HENRY, of Washington, stated that this was a part of the harvest which science had reaped from the great crusade which the British Association had established to investigate the phenomena of terrestrial magnetism. Capt. LEFROY had spent a whole winter, with the thermometer below the freezing point, in making the upper curve on the diagram which he had exhibited here to-day. We had as yet done nothing to compare with this. It was true that there had been a series of observations taken by Dr. Bache, of the College of Philadelphia, which had been published by order of Government; but this was an individual case, and there were no others with which to compare the results obtained. He hoped our own Government would take this matter in hand. There was no people more interested in the practical benefits which such a philosophical investigation would confer, than the people of the United States.

Dr. BACHE wished to mention the name of Col. ABERT in connection with this subject, as one who had most materially aided in making the observations which had been published by Congress, and who was intimately concerned in carrying them on.

Prof. HENRY said he would mention one fact connected with the observations at Toronto and at Philadelphia, and which was, that though the instruments employed at the two places mentioned were very different in construction, each gave the same answer in their indications of magnetic phenomena.

42. On the Meteoric Stone of Deal, New-Jersey, which fell August 15, 1829. By Charles Upham Shephard.

I am indebted to Dr. Elwyn, the treasurer of our Association, for a reference to the notice of the meteorite of Deal by Mr. Robert Vaux and Dr. Thomas M'Euen, published in vol. xvi, p. 181 of the Transactions of the Academy of Natural Sciences (Philadelphia); and still further to the curators of the Academy, for a few grains of the stone, detached from their specimen (of rather more than half an ounce weight), which has enabled me to extend the account of its properties beyond the following brief remark, which is all that is embraced on this head in the paper above referred to, viz : "The stone is three inches in its greatest length, and the surface black with many indentations."

Its sp. gr. = 3,25 ... 3,30.

Its coating is perfectly black, but without the glassy lustre. In some spots, it penetrates by narrow veins and chinks into the mass of the stone for a slight distance.

It is of a light color within (destitute of rust points), and has a vitreo-pearly lustre. Nickeliferous iron is distributed through it in minute shining globules, with here and there bronze-colored specks of magnetic iron pyrites. The stone is slightly coherent, and appears to be destitute of rounded concretions.

The metallic portion is rich in nickel. The earthy part is readily attacked by hydrochloric acid; and the solution formed contains silica, oxide of iron and magnesia, apparently in the proportions of howardite.

The stone may therefore be regarded as nearly identical with that of Castine (May 20, 1848), and of Poltawa (March 12, 1811).

In the course of Prof. Shepard's remarks, the circumstance of the existence of a remarkable meteoric hill in Mexico was mentioned by Dr. Le Conte of New-York. While passing through the village of Tucson, a frontier town of Sonora near the Gila, in February last, he observed two large pieces of meteoric iron, which were used by the blacksmiths of the town for the purposes of an anvil. He was unable to procure any specimens from these bodies, but was guided to a cañon between two mountain ridges in the immediate vicinity, from which both pieces had been taken, where the masses of the

meteorites were so abundant as to have given name to the cañon. He had not before heard any account of this remarkable circumstance, and had considered it an interesting subject for observation.

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43. On the probable date of the fall of the Ruff's Mountain (S. C.) Meteoric Iron. By Charles Upham Shepard.

This highly interesting mass (weight 117 lbs.), first brought into notice by Dr. Thomas Wells, and described by me at the Charleston meeting of this Association, appears to have been one of very recent date. It was brought to the office of Dr. Wells in Columbia in the winter of 1844, with the account that it was incidentally met with by a person out upon a hunting excursion in a somewhat unfrequented place; the position of the mass being that of entire isolation, upon a flat surface of rock. This circumstance, coupled with the fact that its exterior is fresh on all sides and perfectly clean from the hydrated peroxide of iron, seems to justify the inference that it could not have occupied this situation for any length of time; the more especially when it is observed that freshly cut portions are prone to oxidation, even when carefully protected from air and moisture.

The foregoing circumstances have led me to attach considerable importauce to the following very striking description (in a letter to myself) of a meteoric explosion that occurred about forty-five miles to the southeast of Ruff's mountain in the year 1841, and which was communicated to me by Rev. William C. Cooley. It apparently points to the origin of the mass under consideration.

Pottsville (Pa.), *April* 27, 1846.

Dear Sir: In accordance with your wish, I subjoin as accurate an account of the meteoric phenomenon which I witnessed during my sojourn in the South, as my recollection and other more reliable data will enable me to do. It was near the middle of the month of February 1841, that I was a witness to the following impressive events in Richland district (S. C.), on the plantation of the Messrs. Clarksons. As my usual custom then was, I was riding on horseback from one plantation to another (situated about eight miles apart). I had come in sight of a field where the slaves were preparing the ground for the planting of cotton. It was near the middle of the day; and I had slacked my horse's pace, and was slowly descending a hill, when I was suddenly started by a distant whirring sound, very much resembling that made by whirling a shingle through the air

suspended by a string. This sound rapidly grew nearer, louder, and more broken or ragged, until it died away, and was followed by another quite similar, though more distinct, which lasted about the same length of time, and was followed by a third and final report, the most distinct of the three, which terminated in a *crash* like that made by a heavy body falling to the earth. Each report lasted nearly a quarter of a minute, giving fully sufficient time to form an idea of its character. My horse was so completely shocked with fright as to stand stock still for some minutes after the last report had died away, utterly regardless of my efforts to urge him on. The laborers in the field threw down their hoes, and with clasped hands looked imploringly up to heaven, feeling in their benighted souls that the awful day of judgment had come. The whole country for twelve miles around was thrown into a state of great excitement; and every one I met, for days afterward, would immediately ask if I had heard the 'lumbering' in the air. Such is, I believe, an accurate account of what I heard and saw of this phenomenon; of which you are at liberty to make any such use as may be either interesting to yourself, or advantageous to the cause of science.

I avail myself of this opportunity to give publicity to another highly graphic account of a meteoric explosion that took place in Tennessee, the summer after this mass was found. It is from the pen of Judge Voorhies; and should its publicity fail in leading to the recovery of the deposit which probably attended the explosion, the account will serve to strengthen the evidence in favor of the idea advanced on a former occasion, that the States of Tennessee, North and South-Carolina are more liable to these aerial visitations than other portions of this continent.

Charlotte, Dickson Co. (Tenn.), *September* 11, 1845.

Sir: Your letter and circular of 29th August and 28th July last, requesting information relative to meteoric stones and meteoric irons, arrived some days since. We have no account of any meteoric substance having been found in this county, except the mass of iron which I presented to Dr. Troost (that which fell in the summer of 1835). My attention has only very recently been drawn to this subject. Several years ago, I was shown in some part of the country some stones, said to have been precipitated to the earth; but with all my efforts, I am unable to locate the precise time or place, or person communicating the fact. The circumstances accompanying the descent of the mass alluded to, now in the possession of Dr. Troost, induce me to believe that on the last Saturday in the month of July last, at 4 or 5 o'clock P. M., another meteoric substance fell to the earth, some four or five miles from the place where the piece of meteoric iron was found. I was at that time eighteen miles north of the place where I now write, sitting in the piazza of a Mr. Dickson, in company with several gentlemen, all of whom, as well as myself, heard the report. We supposed at first that a large gun had been fired at Clarksville, a town on Cumberland river; but the sonnd was not in that direction. I returned home the next day, speaking with several persons on

the way about the noise they had heard the day before. The citizens of this place also heard the report; and I have understood that it was distinctly heard eighteen or twenty miles south of this. Two white boys from this vicinity were hunting squirrels; and were in the act of shaking a small tree for some purpose, looking upward during their operations, and at that instant saw nearly over their heads a bright light passing rapidly, and fading away quickly. One of them vehemently called out 'Look! look! look!' They describe the light as being of the color of tin. They heard the report, and became alarmed. Every body here says that not a cloud was to be seen. Where I was, the sky was never more cloudless. The boys declare that there was not the sign of a cloud. Two gentlemen were sitting in the piazza of one of the taverns in this town, and both hearing the report, one observed to the other, that the noise very much resembled that made by plunging a hot cannon-ball into a barrel of water; neither of them apprehending, as I understand, what was the cause of the report. A lady who resides five miles south of this place, describing this noise in the heavens, said she was in the house with her father when she heard it, and asked quickly what it was? Her father, who is hard of hearing, said it was thunder. Whereupon she started quickly for the door (observing as she went that it was not like thunder, and that there was no cloud), and looked up. All that she saw was a small white cloud, or patch of whitish smoke about as large as a pocket-handkerchief, which immediately disappeared. Its place was almost over her head, but a little to the east. I will make some efforts to find the fallen mass, if any fell; but if I should not myself, and it should be found by others near the locality I have indicated, it will furnish a presumption that its descent was accompanied by the circumstances I have related. Very respectfully your obedient servant, J. VOORHIES.

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44. An Account of a Meteor which was seen in the vicinity of Hartford (Conn.), on the Night of October 3, 1850. By Prof. J. Brocklesby, of Trinity College, Hartford.

On the evening of the third of October 1850, a splendid meteor of unusual size was seen by two observers, who reside on the eastern slope of Talcott mountain, about seven miles west of the city of Hartford in the State of Connecticut. It was first seen by Mr. Gaylord Wells, and afterwards by his wife; and to the former I am indebted for all the particulars I have been able to collect in respect to this remarkable phenomenon; for I cannot ascertain from the published accounts of meteors, that this brilliant visitant was elsewhere noticed. It would, however, be passing strange, if a body of such vast size, and which appeared so early in the evening and continued visible for so long a time, should have failed in attracting attention; and the silence respecting it must be attributed to the

little interest manifested in the spectacle by those who beheld it. The following facts I took down from the lips of Mr. Wells, with whom I have been acquainted from my boyhood, and whose statements as to what he saw I know to be worthy of the utmost reliance.

The place where my informant resides commands a full view of the heavens in three directions : north, east and south. On the night in question, he stepped out of the eastern door of his house at about half past eight o'clock, as near as he could judge : the sky was serene, and the moon within about an hour of the meridian. Upon passing round the southeast corner of his house, Mr. Wells saw, a little south of west, and full sixty degrees above the horizon, a bright meteor apparently *a foot* in diameter. It shone with an orange hue; and below it was a train which seemed to be *fifteen or sixteen feet in length*, fan-shaped, and possessing an apparent breadth at its further extremity of *full two feet*. The train shone with a mild phosphoric lustre, and resembled a light and delicate summer cloud. The meteor rose from west to east with a slow and stately motion; the train preserving nearly its original length as the body advanced towards the meridian, and swept onward to the moon. In its progress the meteor passed above, or to the north of this luminary; and when it had arrived on the eastern side, directly turned towards the southeast, and, dropping down below the moon, a part of its attendant train *swept over the lunar disk*. As it crossed it, the face of the moon was slightly obscured, as when dimmed by the passage of a fleeting cloud. The meteor now gradually descended, and was watched until it had reached the verge of the horizon in the southeast; and, when last seen, appeared, together with its train, to be not more than eight or nine inches long. Neither explosions nor scintillations were observed in any part of its course, and it appears to have been unattended with any remarkable changes in form.

As far as any judgment could be formed of the velocity of this body, it is believed that the time occupied in moving the length of its train could not have been less than three minutes. The duration of the visibility of the meteor is not accurately known, as the observer did not refer to the clock at the beginning and end of the phenomenon; but he is positive that it could not possibly have been less than *an hour*, and was probably *an hour and a half*. In truth, Mr. Wells staid out so long gazing upon the wondrous spectacle, that his wife came out to see what had become of him, and a severe cold was the result of his protracted exposure.

I regret that my informant was unable to give me the angular measurement of this meteor and its train, if it was only for the sake of comparing its dimensions with those of other meteors; but in one respect this déficiency is partially supplied. The meteor of September 30th, 1850, which has been well described by Prof. BOND, was also observed by my informant when near the Pleiades. He considers the meteor of October 3d, 1850, to have been *much larger* than this, when seen near the stars just mentioned; but that the September meteor was superior in brilliancy to the one of October 3d. They probably differed but little in respect to the duration of their visibility. It is remarkable that two meteors of such extraordinary size, and which continued above the horizon for so long and unprecedented a time, should have swept through the heavens over the same places on the earth within three days of each other; and, unless we knew that their paths were different, we might almost be tempted to imagine that they were kindred bodies circling as companions through the fields of space.

B. CHEMISTRY AND MINERALOGY.

I. CHEMISTRY.

1. ANALYSIS OF THE MUSKMELON (*Cucumis melo*), AND WATERMELON (*Cucurbita citrullus*). By J. H. SALISBURY, M. D., of Albany.

THE varieties examined were the *Nutmeg Muskmelon* and the *Long Red-flesh Watermelon*. The fruit only was examined. Length of muskmelon, 6 inches; diameter, 5¾ inches. Length of watermelon, 14 inches; diameter, 6 inches.

PERCENTAGE OF WATER, DRY MATTER, AND ASH.

	Muskmelon.	Watermelon.
Percentage of water	90,987	94,898
— dry matter	9,013	5,102
— ash	0,271	0,248
— ash in the dry matter,	3,007	4,861

The muskmelon contains but a trifle more water than the beet: the watermelon contains more than the muskmelon, and less than the cucumber. One ton of the fresh fruit of the muskmelon has 174,84 lbs. of organic matter, and 5,42 lbs. of inorganic matter. One ton of watermelon fruit, fresh, contains 97,08 lbs. of organic matter, and 4,96 lbs. of inorganic matter. 36900 lbs. of muskmelons and 40322 lbs. of watermelons contain each 100 lbs. of inorganic matter or ash.

	100 lbs. ASH of Muskmelon.	100 lbs. ASH of Watermelon.
Carbonic acid	11,55	11,42
Silicic acid	2,20	1,21
Phosphoric acid	25,40	14,93
Sulphuric acid	3,90	1,63
Phosphate of iron	2,30	4,52
Lime	5,85	7,32
Magnesia	0,60	1,31
Potash	8,35	23,95
Soda	34,35	30,63
Chlorine	5,20	1,81
Organic matter	trace	trace
	99,70	98,73

The muskmelon contains a very large percentage of phosphoric acid and soda, and considerable potash: the watermelon has a very large percentage of soda and potash, and is also quite rich in phosphoric acid. The occurrence of these bodies in such quantities in these plants explains to us why dead animal matter, as flesh, bones, etc., and common salt and ashes, have such a marked influence in promoting their growth and productiveness.

PROXIMATE ORGANIC ANALYSIS OF FRUIT.

	100 lbs. of Muskmelon.		100 lbs. of Watermelon.	
	Fresh fruit.	Dry fruit.	Fresh fruit.	Dry fruit.
Albumen	0,918	10,219	0,572	11,403
Casein	0,442	4,952	0,004	0,080
Dextrine	1,142	12,800	0,318	6,340
Starch	trace	trace	none	none
Sugar and extract	5,250	58,942	3,020	60,267
Chlorophyl	0,004	0,044	0,006	0,120
Fat, wax, and resin	0,038	0,415	0,022	0,440
Citric acid	trace	trace	0,007	0,140
Malic acid	0,007	0,077	0,009	0,180
Tartaric acid	0,005	0,055	trace	trace
Fibre	1,123	12,393	1,058	21,030
Dry matter	8,929	100,	5,016	100,
Water	90,987	......	94,898	......
	99,916			

The large percentage of albumen, casein, dextrine and sugar, with

a small quantity of acid, shows us the reason of the peculiar rich flavor of the fruit of the melon.

ULTIMATE ORGANIC ANALYSIS.

100 parts of dry fruit of the	Muskmelon,	Watermelon,	yield of
Nitrogen	2,231	1,739	
Oxygen	43,905	43,187	
Carbon..........	44,820	43,764	
Hydrogen	6,832	6,872	

The melon furnishes a mild but very pleasant liquor : for this use, the muskmelon is much superior to the watermelon.

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2. On the Separation of Butter from Cream by Catalysis. By President Edward Hitchcock.

It is well known that the separation of butter from cream, during the winter months, by the ordinary process of churning, is often very difficult, from some chemical changes in the proximate principles. From my own small kitchen dairy, the complaint on this subject had so often reached me, that I was led a few years since to inquire whether there were not some remedy. My thoughts were turned to that principle in chemistry, to which Berzelius gave the name of *catalysis.* In observing the process of churning with the old-fashioned cylindrical churn, I had noticed that along the handle, when the cream had been subject to a more powerful agitation, butter would show itself much earlier than in the body of the cream. Hence I inferred that by acting on a small quantity of the cream, the separation might be easily effected in that portion : and it seemed not improbable, that by seizing the exact moment when the separation was taking place, and adding more cream, the process might be communicated to that also in a catalytic manner ; and if so, perhaps any quantity might in like manner be made to yield its butter.

I made the experiment, and was successful. I put a small quantity of cream in the churn at first, and, by a few moments strong agitation, brought it to that state, familiar to a practised eye, when the butter is separating. An assistant stood with the principal mass of the cream, ready to pour it gradually into that where the butter was in a nascent state, which I continued to agitate with even increased briskness as more and more cream was added. The effect was ma-

gical; for in a few minutes, I several times had the pleasure of seeing several quarts of cream give up its butter. I found, however, that if the fresh cream were poured in too fast, it would stop the process; and that it would not answer to let the agitation cease for an instant.

I have delayed for two or three years to state these facts publicly, because I had hoped to make additional experiments on the subject; but more important matters have prevented. I cannot, therefore, say of how much practical value my statements may be. I tried experiments enough to convince me, that although the requisite manipulations would require some skill, it would not be greater than many other processes common upon farms. The common churn, however, is not adapted to the experiment. I think one might be invented that would meet the case; but I must leave the whole matter to any others, who may feel interest enough in it to carry forward what I have only suggested.

I ought to add, that it was suggested to me by some who took charge of the butter thus eliminated, that it seemed more difficult to separate the butter and the whey completely, than when obtained by the ordinary process. To this point, therefore, the attention of the experimenter should be turned.

It may be thought that this paper would more properly be presented to an agricultural than a scientific association. I offer it to the latter, because it was scientific principles alone that led to the experiment; and, besides, I know not as yet whether it will be of practical value to the farmer. It is, however, a new example of catalytic change, and therefore worthy of record in the annals of science.

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3. Analysis of Bituminous Coal Ash. By George W. Weyman, of Yale College, New-Haven.

The analysis of anthracite coal ashes, presented by Mr. Bunce at the New-Haven meeting of this Association, having given so interesting results, I was led to undertake a similar investigation of the bituminous coal ash.

The coal from which the ash was obtained was procured at Pittsburgh (Pennsylvania), and is the same as is generally used there.

It was burned in a large furnace where the heat was intense, and every precaution was taken to have it free from any impurity. The fire was allowed to burn for a day, and then thoroughly raked out before any ash was taken.

The mean of three determinations of ash, etc. in the coal, gave the following results :

Bitumen	32,32
Carbon......	64,34
Ash	3,34
	100,00

Three determinations of the amount of the ash soluble in water and hydrochloric acid, gave these results :

	First determination ;	Second ;	Third.	Mean.
Soluble in water	3,40	3,41	3,42	3,41
Soluble in hydrochloric acid,	8,53	8,36	8,46	8,45
Insoluble	88,06	88,06	89,79	88,63
	99,99	99,83	101,67	100,49

The amount soluble in water agrees very nearly with the solubility of the anthracite ash ; while that soluble in acid is only a little more than half as much.

A qualitative analysis detected the presence of silica, iron, alumina, lime, soda, potash, sulphuric acid, and chlorine.

Separate quantitative analyses were made, both of the portion soluble in water and soluble in hydrochloric acid.

PORTION SOLUBLE IN WATER.

	First ;	Second.	Mean of the two.
Silica	5,243	5,076	5,1595
Alumina and iron,	0,972	0,461	0,7165
Lime	52,811	53,630	53,2200
Soda............	6,439	6,334	6,3865
Potash	10,371	10,156	10,2635
Chlorine	4,680	4,730	4,7050
Sulphuric acid ...	18,590	18,206	18,3980
	99,106	98,593	98,8490

The quantity of iron and alumina in the above was so small, that they were weighed together.

PORTION SOLUBLE IN ACID.

	First;	Second.	Mean.
Silica	10,994	10,169	10,5815
Iron	18,150	19,801	18,9755
Alumina	23,734	22,687	23,2155
Lime	31,860	31,664	31,7620
Soda............	7,131	5,763	6,4470
Potash	2,035	1,968	2,0015
Sulphuric acid ...	5,850	5,600	5,7250
Chlorine	1,203	1,964	1,5835
	100,957	99,616	100,2915

It will be observed in the above, that all of the alkalies was not dissolved by water, and about a third of them was determined in the acid solution. The ash was washed with water until the water gave no taste. The alkalies existed undoubtedly in the state of silicates.

Calculating the above determinations on the whole ash, we have this result; and for the sake of comparison, I give Mr. Bunce's result also.

	BITUMINOUS.	ANTHRACITE.	
		Red-ash;	White-ash variety.
Silica	1,0599	1,237	0,091
Alumina	1,9650	4,244	3,315
Iron	1,5862	5,828	4,028
Lime	4,4647	0,159	2,111
Soda............	0,7560	0,164	0,218
Potash	0,5161	0,105	0,162
Sulphuric acid ...	1,1041	0,430	0,865
Chlorine	0,2922	0,013	0,087
Phosphoric acid ..	......	0,269	0,198
Magnesia	......	2,008	0,195
Insoluble........	88,0600	85,647	88,681
	99,8042	100,104	99,951

But the slightest trace of phosphoric acid was detected in the bituminous ash. By comparing this ash with that of the anthracite, we find that this is much more rich in alkalies : in the anthracite they amount to about 0,4 of a per cent, while here they exceed one per cent. The lime is also about twice as much. No magnesia was detected. The absence of phosphoric acid in this ash lowers its value as a manure; but the presence of so large a quantity of the alkalies compensates, in some measure, for its absence. These results fully confirm the value of coal ashes, and their applicability as a manure.

4. On the Value of Soil Analyses, and the Points to which especial attention should be directed. By Prof. John P. Norton of Yale College, and William J. Craw, First Assistant in Yale Analytical Laboratory.

The object of this paper is to show that distinct practical results may be obtained by the careful analysis of soils; results from which conclusions may safely be drawn, and which point clearly to certain leading principles in agriculture. It may be thought strange that demonstration should, at this advanced period of improvement, be required in such a cause; but the fact is that many chemists, and among them some of the highest reputation, have of late been accustomed to speak slightingly of soil analyses, and to intimate that they are of little value. Some have even said that we cannot from analyses tell what constitutes fertility, and what barrenness. It has also been said that we can learn more by analysing the ash of weeds or other plants growing wild on a soil, than by analysing the soil itself.

With all proper deference to the authority of others, the assertion may safely be made that such as these are always founded upon erroneous ideas with regard to the connection between the soil and the plant, and also to deficiency in practical knowledge. This last is especially an important point : those who work only in the laboratory upon small quantities, and in accordance with scientific theories, can have little true conception of the different circumstances which they must meet, or rather their theories must meet, when brought into the open air; when tons are in question, rather than milligrammes; when sun, wind and rain have full scope; when the winter's frost and summer's heat unite in the work of change and transformation. Thus it is that many have been disappointed in their anticipations, and have given up the whole matter in despair, when a more cautious adoption of theories, and a due consideration of actual realities, would have led to far different results.

Another class of objectors find fault with analyses of soils, as not affording after all a fair index to the true composition of any given soil. The analysis is made upon a minute quantity; and this, in separating its constituents, is divided into yet more minute quantities, so small that, as it is said, they have an extremely uncertain relative proportion to the great unhomogeneous mass from which the original

sample was taken. This objection is one\chiefly urged by accurate chemists, who are accustomed to analyse minerals and other bodies having a perfectly fixed constitution, and to make determinations on different specimens corresponding almost precisely with each other. From such a source the objection is certainly entitled to respect, and to it attention will be first directed.

No pretension is urged that analyses of soils may be made to agree as closely as those of minerals : this is, from the nature of the case, impossible ; but the agreement is ordinarily quite sufficient for the practical purposes intended. As the strongest possible proof of this, two samples of soil were taken from widely separated parts of the same field, one of six acres. These analyses were made with a view of determining certain points, and are therefore not carried out so fully as is usual in Yale Analytical Laboratory. Neither the iron and alumina, or the potash and soda, were separated. The analyses are as follows :

TABLE I.

	TRAP.		RED SHALE.	
	No. 1.	No. 2.	No. 3.	No. 4.
Iron and alumina...........	9,509	8,645	10,884	15,344
Lime	0,314	0,216	0,247	0,074
Magnesia	0,865	0,716	0,771	1,728
Sulphates of potash and soda,	1,121	0,717	0,700	2,915
Soluble silica	0,166	0,127	0,237	0,122
Insoluble siliceous matter....	81,677	82,070	82,581	75,887
Organic matter.............	7,167	7,760	5,058	4,457
	100,819	100,251	100,478	100,557

These analyses overrun a little uniformly, for the reason that the alkalies were not especially examined and separated : they undoubtedly contained a little baryta from the separation of the magnesia, and are somewhat too high. The general coincidence between these soils is quite decisive, without being perfectly accurate. It is such a coincidence as one would expect, and it is perfectly obvious that all practical information for the improvement of the field might be as well drawn from the study of one as from the other. These were both trap soils ; a third trap soil was taken from a field perhaps a third of a mile distant from the first, and the analysis is No. 3 in Table I. Here again we have the same leading features, the same decisive similarity in composition. In view of this coincidence, is it not perfectly obvious that a few pounds of soil from four or five places in a field, mingled together thoroughly, will give a very

satisfactory mean as to the composition of the whole field, a mean quite sufficiently accurate for practical purposes? The same points may be exemplified by the following analyses of some clays from the vicinity of Albany :

TABLE II.

	No. 1 : blue.	No. 2 : blue.	No. 3 : white.
Alumina and iron	13,22	13,76	17,01
Lime	4,87	4,83	4,31
Magnesia	2,47	2,69	2,71
Potash and soda.........	3,67	3,16	4,80
Soluble silica	1,68	1,19	0,47
Insoluble siliceous matter,	70,40	69,16	67,63

The clays No. 1 and No. 2 were taken from a hillside near Albany, but at a considerable distance apart : their appearance was quite alike, and the agreement in the analyses is even more striking than in the case of the trap soils, there being in no case a difference of more than half a per cent between any of the determinations. They might very well be published as accurate determinations of the same specimen. The white clay from the same neighborhood, No. 3 in the table, was a decidedly lamellar clay, from a much lower layer of the same series, and a locality considerably removed from the first. The degree of coincidence which it shows with the first two samples is quite remarkable : it is in all essential respects the same soil, the only very marked difference being in the amount of iron and alumina and of soluble silica. The same remark made as to practical deductions from the study of analyses, will apply here as in the previous instance of the trap soils : the three are obviously of the same class.

If now we compare with these soils others taken from different formations, we shall find equally satisfactory results. In Table I is included an analysis of a red-sandstone shale taken from within an eighth of a mile of the three trap soils : in fact, this shale appears between soil No. 1 and 2 and No. 3. The difference in composition is very marked : the amount of soluble matter is greater by six or seven per cent; the alkalies, by nearly two per cent; the magnesia, about one; and lime is almost absent, there being but about 1½ hundredths of one per cent.

These four soils were all from Farmington (Connecticut). Within half a mile or less of their locality lies the soil of the gravel and sand drift, which here stretches quite across the State and into Massachusetts, running at least ninety miles almost unbroken. This forms a poorer soil, differing widely from either of the others, or from the

clays given in Table II. In the following table are the results of analyses of two of these from the neighborhood of Newhaven, but on precisely the same formation as exists at Farmington.

TABLE III.

	No. 1.	No. 2.
Iron and alumina........	6,659	5,117
Lime	0,099	0,079
Magnesia	0,350	0,283
Potash and soda.........	0,384	0,454
Silica	0,335	0,264
Insoluble siliceous matter,	90,456	91,865
Organic matter..........	2,172	2,280
	100,454	100,342

The proportion of insoluble matter is greater by some 20 per cent than in the clays, and by 10 to 15 per cent than in the other soils. Lime is very trifling in amount, and magnesia or alkalies in neither case amount to half of one per cent. A soil formed by the decomposition of chlorite and serpentine, the locality not more than half a mile distant from these sandy soils, was partially examined, and found to show a very different character; abounding especially in lime, magnesia, and soluble silica.

It was intended to present a larger number of results than those at present completed; but various circumstances conspired to frustrate this intention, as also to prevent the analyses from being carried out more fully. As they stand, however, they confirm in an unexpectedly satisfactory manner the views entertained by the authors. They are not selected analyses; all that were made being published, with a single exception, that one being rejected on account of an obvious defect.

These analyses, having been only carried so far as to serve a particular purpose, are somewhat rough and unfinished in their character, and may be found fault with in that respect. To show that minute particularity does not lessen, in all cases at least, the practical value of analytical results, two analyses are here inserted, which were made in Yale Analytical Laboratory a few months since by Messrs. Erric and Brewer : they are of soils from the cotton lands of Mississippi.

TABLE IV.

		A.	B.
	Organic matter	4,740	6,290
Soluble in water. A. 2,470 per cent. B. 0,147 "	Silica	1,299	0,072
	Iron, alumina and phosphates,	0,230	0,019
	Lime	0,389	0,020
	Magnesia	0,090	none
	Manganese	0,034	none
	Potash	0,248	0,120
	Chloride of sodium	0,107	....
	Soda	....	0,015
	Sulphuric acid	0,144	0,009
Soluble in acid. A. 4,96. B. 5,19.	Silica	0,409	0,920
	Alumina	1,644	1,820
	Iron	1,448	0,670
	Lime	0,585	1,340
	Magnesia	0,576	0,080
	Manganese	0,002	none
	Potash	0,348	0,070
	Soda	....	0,180
	Sulphuric acid	0,070	0,080
	Phosphoric acid	0,042	0,008
Insoluble portion. A. 87,88. B. 88,878.	Silica	78,845	84,930
	Iron and alumina	5,946	2,370
	Lime	1,098	0,260
	Magnesia	1,142	0,680
	Manganese	0,623	none
		100,059	99,867

All of the determinations in these analyses are the mean of at least two nearly agreeing results, and they may therefore be looked upon as singularly accurate and exact.

A is from a new strong cotton soil. B was originally the same soil, but has been worn out by long cultivation. The analyses speak for themselves. In B, the portion soluble in water has been exhausted; in the other portions, its inferiority is not so decisively marked. From the inspection of these analyses, without any knowledge of the composition of the cotton plant, the opinion was expressed that the special manure needed by the worn-out soil was alkaline phosphates. Since that time, the ash of a cotton stalk from Mississippi has been analysed in Yale Analytical Laboratory by Mr. Judd, and this analysis is presented at this meeting : it confirms the above opinion in a very remarkable degree, so far as that portion of the cotton plant is concerned.

In view of all the foregoing statements and results, it is claimed that they go to sustain in a most decisive manner the value of soil

analyses. They show that even somewhat rough examinations are, or often may be, highly useful in a comparative point of view; and that in some instances, certainly, the utmost minuteness of investigation only renders the results more satisfactory. In fact, these careful analyses are the only ones that can be relied upon in any difficult or doubtful cases. It is easy to see that had not a separate analysis been made of the water solution in the above two cases, the deductions from the analyses would have been quite obscure and uncertain; there would have been no *very* marked difference perceptible between these soils.

It is necessary to state that all analyses do not afford conclusions alike satisfactory: there are cases which are yet obscure; cases upon which the most careful investigations yet made have failed to throw full light. We need careful and detailed analyses in great numbers; analyses in which each determination is verified, and where all the circumstances of climate, temperature, course of cropping, etc. etc. are taken into consideration. From the comparison of such analyses, great results may be expected.

It may not be inappropriate to indicate some of the points to which, in the present state of our knowledge on this subject, special attention should be directed.

First as to the organic matter: this should always be determined carefully; and probably it may be of advantage in many cases to determine how much is soluble in alkalies, and is therefore immediately fit for the food of plants. In most cases, however, a knowledge as to the wetness or dryness of the soil will give information upon this point. If the soil is properly dried, decomposition of its vegetable part will undoubtedly proceed with sufficient rapidity for all the purposes of the plant. Our knowledge of the organic bodies that exist in the soil is quite indefinite, and we look with suspicion on all determinations of crenates, apocrenates, ulmates, humates, ulmin, humin, etc. Years of patient investigation are required to bring our knowledge in this department of agricultural science into a proper shape for practical use.

Neither are we inclined to place any particular reliance on what is called the property of absorption, upon which many chemists lay much stress. In almost all cases a practised eye will decide, from a mere inspection of an analysis, whether a soil has much absorbent power or not.

Particular attention should, in all critical cases, be paid to the portion soluble in water. A separate examination of this portion greatly increases the labor of analysis; but that labor is always well expended, for it shows upon what the plant can rely for its immediate nourishment.

The acid solution should be made by heating with dilute acid for a definite length of time, digesting, not boiling. If these points are not attended to, the acid solution will vary, owing to the unequal quantities of iron and alumina, and of soluble silica, dissolved.

It is almost unnecessary to direct particular attention to the phosphates; as no one, who has worked at them critically, will need to have them recalled to his memory. They are of vital importance to the plant, and their presence should always be ascertained. We have at present no method for the separation of phosphoric acid from the bodies with which it is associated in soils, that can be confidently recommended. Beyond a doubt, the majority of the determinations which we possess are mere approximations. The analyst should never rest satisfied with less than two or three nearly concurring determinations; and those, in our experience, are not easy to obtain. The grand desideratum at present in the inorganic part of this class of analyses, is a method for the exact and speedy determination of phosphoric acid.

Much attention is to be paid to the alkalies also: in critical cases, they should always be determined separately; for when the joint weight by evaporation is relied upon, without any farther proceeding, the result is almost invariably too high, as exemplified in some of the analyses given in this paper.

Analyses of the insoluble portion are also useful, when a complete examination is desirable; because the substances there contained are, without doubt, undergoing gradual changes, which will fit them for the food of plants, and may be regarded as a store for future consumption.

It is probable from the recent results of Way, and also of Anderson, that much benefit may often result from ultimate determinations of nitrogen, with testing for the purpose of ascertaining whether it exists chiefly in the form of nitrates or ammoniacal salts. This last, however, is a point of theoretical interest, as the practical effect seems to be the same in either case.

Lastly, the different circumstances of physical character, elevation, climate, wetness or dryness, and the previous course of treatment,

should all be carefully considered when the analysis is completed; for upon some of these points, and not upon the composition as shown by destructive analysis, the explanation of difficulties depends. The idea that any ordinary chemist can prescribe for the complaints of a soil, by rule and measure, from a mere examination of a small dried sample, without further knowledge of it, is utterly absurd, and can, as it is now being acted upon in many parts of our country, only lead to most serious consequences.

It is the duty of members of this Association, whatever may be their department, to frown upon all false science; and in no department are their exertions more needed than in this. A multitude of analysts are springing up in various parts of the country, following the daily increasing demand; and, by floods of cheap, worthless analyses, are injuring a noble cause to an immense extent. Some err from ignorance: others are mere pretenders; but all should alike be discouraged. Cheap analyses, where the operator depends on them for his support, must be slighted somewhere.

It is a matter of vast national importance that the deterioration of land under an exhausting and wasteful cultivation, which is going on more or less in every part of this country, should be checked speedily. Every American of every station is directly interested in a subject which affects the vital prosperity of our country; but every scientific man is particularly interested, because it is science that must effect this change by its teachings; and when completed, all true science and all scientific bodies will gain a higher place than they have ever before occupied in the minds of the great mass of this nation. When we can show the true practical utility of science so widely and so beneficently in one branch, all other branches will also receive a share of confidence and support. Then, too, on the other hand, when one branch of science suffers from misrepresentation and falsification, all suffer more or less with it. One office, therefore, of this Association, and of every member in his own place, is to encourage the true and sound — to discourage all that is ephemeral and superficial.

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5. On a New Method for the Analysis of Soils. By David A. Wells, Junior.

[*Not received.*]

6. Comparative Analyses of Ash from Premium Samples of Ash of 8-rowed Yellow Indian Corn. By Mason C. Weld, of the Yale Laboratory.

[*Not received.*]

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7. Solidification of the Rocks of the Florida Reefs, and the Sources of Lime in the Growth of Corals. By Professor E. N. Horsford, of Harvard.

I. It is required to ascertain by what processes, chemical or mechanical, or both chemical and mechanical, the surface and the submerged rocks have become hardened.

By the surface rock is intended that thin brown crust, composed of numerous layers, which is distinguished by great compactness, and a peculiar ring, when, in detached condition, it is struck by a hammer, and which occurs on the abrupt ocean side, and more abundantly on the long slopes on the land side of the Keys.

By the submerged rock, is intended the rock of oolitic appearance which has solidified under water, and which is of inferior hardness to the surface rock.

The surface rock, so called, has in many places no longer the outermost position, though it had at the time of its formation. It is indeed interstratified with friable light-colored limestone. The epithet indicates the circumstances of its formation, not its present position.

1. We are familiar with the fact that a mixture of quicklime, water and sand, spread out upon walls and ceilings exposed to an atmosphere containing more or less of carbonic acid, in a few days becomes hard. Analyses have shown that two chemical phenomena are concerned in the solidification, to wit : the absorption of carbonic acid from the air, forming carbonate of lime (which salt, uniting in equivalent proportions with the hydrate, forms, according to Fuchs, a compound of great stability); and the union of the outer portions of the sand-grains with the lime, forming a silicate. Investigation has shown that sand fulfils mechanically a more important office, by increasing the extent of surface to which the compound of the hydrate

and carbonate may attach itself. The latter office may also be performed, and equally well, by pulverized limestone.

II. It is well known that calcareous springs deposit carbonate of lime in crystalline forms. The salt had been held in solution by carbonic acid contained in the water. Upon reaching the surface under less pressure and the influence of a high temperature, its carbonic acid is given up, and with it a precipitate of carbonate of lime takes place. The process is exclusively chemical.

III. The value of hydraulic cements is now conceived to depend chiefly upon the presence of silica and lime, the oxide of iron having little or nothing to do with the process of solidification. The alumina, in the form of a silicate, yields its silica to the lime, which, for its transportation, requires water. This explains the necessity of its being retained under water periods of variable length, according to the proportions of the ingredients. The processes are both chemical and mechanical.

IV. Gypsum from which two atoms of water of crystallization have been expelled by heat, rapidly hardens upon being mixed with water. This is ascribed to the reunion of the sulphate of lime with the water.

Do either of the above processes suggest the method by which the rocks of the Florida reefs have been hardened?

The facts presented in the furnished specimens are as follows:

The rock formed under water exclusively is composed of grains of size less than that of a mustard seed, which, to the naked eye, appear quite globular and of uniform diameter. More carefully examined with a microscope, they are found to be far from regular in form or uniform in size, but present numerous depressions and prominences. Distributed throughout the intervening spaces is a fine deposit of carbonate of lime, which adheres with considerable tenacity to the surface upon which it rests.

The surface or crust-rock, though not strictly homogeneous, is composed of particles so minute as not to be distinguished from each other. It dissolves in hydrochloric acid, leaving a flocculent residue. The solution, when evaporated to dryness and ignited, readily redissolves in hydrochloric acid, with only an occasional residue. The solution gives no precipitate with chloride of barium. Nitrate of silver gives, in a nitric acid solution, a white precipitate soluble in ammonia. The aqueous extract gives to alcohol flame the characteristic soda tint. The powdered rock, dried at 100° C., when heated in a dry tube, gives off water.

Thus the qualitative analysis of the incrusting rock showed it to consist of lime, soda, carbonic acid, hydrochloric acid, water, and organic matter. There were also variable traces of peroxide of iron, magnesia and silica. The former two were wanting in most of the specimens examined, and the silica in some. Numerous specimens were examined for alumina, without in any instance finding a trace of this substance.

In a quantitative analysis by HOMER :

I. 0,255 grammes of substance gave 0,2330 gr. of carbonate of lime.

II. 0,1745 gr. of substance gave, with a Will and Fresenius apparatus, 0,06 gr. of carbonic acid.

III. 0,172 gr. of substance gave, with the same apparatus, 0,0585 gr. of carbonic acid.

IV. 1,289 gr. of substance gave to a chloride of calcium tube, with the aid of heat and an aspirator, 0,028 gr. of water.

V. 0,591 gr. of substance lost by prolonged ignition in a platinum crucible, 0,260 gr.

VI. 0,685 gr. of substance gave 0,0101 gr. of chloride of silver, corresponding with 0,0025 gr. of chlorine, and with 5,806 per cent of chloride of sodium.

VII. 0,376 gr. of substance gave 0,0005 gr. of silica.

Deducting the water and carbonic acid from the total volatile matter, we have the organic matter = 6,92 per cent. It is conceived that all the chlorine present is in combination with sodium*.

The above determinations, expressed in percents, give

	I.	II.	III.	IV.	V.	VI.	VII.	
Lime ...	51,17	——	——	——	——	——	——	51,17
Carbonic acid ..		34,38	34,01	——	——	——	——	34,24
Water				2,17	——	——	——	2,17
Chloride of sodium					5,81	——	——	5,81
Silica						0,01	——	0,01
Organic matter							6,92	6,92
								100,32

These ingredients permit no action like that occurring in hydraulic cements, in which silica plays an important part; or like that presented in the hardening of gypsum, in which sulphuric acid is necessary. To one of the two remaining processes, if to either, must

* As soda was recognized by the flame, and as the lime is equivalent only to the water and carbonic acid, the chlorine has all been referred to the sodium. If a portion were in combination with calcium, it would not materially affect the percentage results, and not at all the view taken of the cause of solidification of the reef.

it be ascribed; and as hydrate of lime is present, it cannot be exclusively assigned to a place with calcareous spring deposits. Now how could hydrate of lime be provided from carbonate of lime?

The completeness of the suite of collections provided for me by Prof. AGASSIZ has enabled me to answer this question in such a manner as leaves, I think, little room for doubt. On the main land against the Keys, there are depressions which are filled with water only at long and irregular intervals. This water, like that within and about the keys, abounds with animal life. As the water evaporates, these animals die, and fall upon and mingle with the coral mud at the bottom. As the beds become more and more completely dry, the layer of mud and animal matter hardens till it forms a mass resembling the surface or crust rock.

Of this soft, growing rock, specimens were collected. Agitated with water, it yielded a turbid, fœtid solution. Tested with acetate of lead, it betrayed the presence of hydrosulphuric acid. After standing some hours, a delicate white film was deposited upon the containing vessel, at the surface of the water, which proved to be carbonate of lime. Test-paper showed the liquid to be alkaline. The addition of soda solution set ammonia free, and the addition of chloride of barium and hydrochloric acid showed the presence of sulphuric acid.

Conceiving this soft rock to be in the condition in which the solidified crust was at first, the process of hardening seemed of easy explanation.

The animal matter mixed with the carbonate of lime, containing sulphur and nitrogen, besides carbon, hydrogen and oxygen, in the progress of decay, which warmth and a small quantity of water facilitated, gave, as an early product of decomposition, hydrosulphuric acid: this, by oxidation at the expense of the oxygen of the atmosphere, became water and sulphuric acid. The sulphuric acid coming in contact with carbonate of lime, a salt soluble in 10,600 parts of water, resolved it into sulphate of lime, a salt soluble in 388 parts of water. The carbonic acid set free, uniting with an undecomposed atom of carbonate of lime, rendered it soluble. The nitrogen going over into the form of ammonia, at a later period, decomposed the sulphate of lime, forming sulphate of ammonia and soluble hydrate of lime. This hydrate of lime, with an atom of carbonate of lime, united to form the compound in ordinary mortar investigated by Fuchs. The carbonate of lime in solution from the added carbonic acid, as the

water is withdrawn by evaporation, takes on the crystalline form, giving increased strength and solidity to the rock.

That this explanation may serve, in however small measure, for the crust rock on the land slopes of Key West and all localities of a similar character, it is necessary that there be animal exuviæ in coral mud, or finely divided carbonate of lime. Both these occur. The water about the Keys abounds in animal life.

With the influx of the tide, the slopes become overspread with the water and what it contains in suspension. The retreating water, at ebb tide, leaves a thin layer of the animal matter, mixed always when the water is agitated with the fine calcareous powder. Before the return of flood tide, exposure to the atmosphere and warmth have secured the succession of chemical changes enumerated above, and a thin layer of rock is formed. A repetition of this process makes up the numerous excessively thin layers of which this rock is composed.

On the ocean side the deposit is formed from spray, during winds which drive the froth of the sea, containing, with coral mud, the exuviæ from the barrier of living corals upon the low bluffs of the keys.

To these chemical changes must be added the simple admixture of the animal and vegetable matter, which, like mucilage or glue, fills up the interstices, increases the extent of surface, and with it the cohesive attraction; and still further to the decomposition of the organic matter furnishing carbonic acid, which gives solubility to the pulverulent carbonate of lime.

The exceeding fineness of the coral mud is due in part to the stone plants which flourish in the waters within the reef, and which admit of ready reduction to a powder of extreme fineness. Of these, two species of Millepora and one of Opuntia were analysed by Scoville in my laboratory.

Opuntia gave of

Organic matter.....	4,18	5,72
Carbonic acid	37,68	35,81
Lime	51,81	51,36
Water	5,59	5,92
	99,26	98,81

MILLEPORA gave of

Organic matter.....	4,45	4,45
Carbonic acid......	40,09	39,64
Sulphuric acid.....	0,0056	0,0056
Lime.............	47,71	47,98
Water............	3,67	3,30
	99,9156	99,0756

Another MILLEPORA gave of

Organic matter.....	1,26	2,58
Carbonic acid	41,08	2,70
Lime.............	46,35	46,80
Magnesia	6,23	5,90
Water	4,52	
	100,44	

The discrepancies in the analyses of the different specimens of the same species are due to the circumstance that different parts of the stone plant contain organic matter in unlike proportions; and it is very difficult to procure two specimens which, when pulverized, will present homogeneous powders of the same constitution.

II. SOURCE OF LIME IN THE GROWTH OF CORALS.

Marcet*, as early as 1823, observed carbonate of lime in the sea water near Portsmouth. Jackson† found it in two specimens of sea water furnished by the United States Exploring Expedition; one from 600 feet, and the other from 2700 feet below the surface. J. Davy‡ found the sea-water of Carlisle Bay, Barbadoes, to contain about $\frac{1}{10000}$th part of carbonate of lime. There was found scarcely a trace near the volcanic island of Fayal. White|| is of the opinion that it fails only near the surface; but the elaborate analysis by Bibra§, of no less than ten specimens taken generally from a depth of twelve feet, but in one instance from a depth of four hundred and twenty feet, in various latitudes on both sides of the equator, shows quite conclusively that it is not a constant ingredient of sea water.

* Annals of Philosophy, April 1823, p. 261.
† Am. Jour. Science, 2d S., Vol. v, p. 47.
‡ Phil. Magazine (3), xxxv, p. 232.
|| Id., p. 308.
§ Ann. de Chemie et de Pharmacie, lxxvii, 90.

His analyses do not mention a trace of carbonate of lime. The quantity found by Davy is very nearly that which is soluble in water, and is obviously due to the calcareous marl which abounds near the Barbados.

The water from within the Keys was carefully analysed in my laboratory : it contained lime and sulphuric acid among its ingredients, but not a trace of carbonic acid.

The total want of carbonic acid in a water in which coral life is so luxuriant, suggests naturally that the stone plant, as well as the coral animal, possesses the power of abstracting lime from sulphuric acid ; the change being due to double decomposition with carbonate of ammonia excreted from the plant and animal, yielding carbonate of lime, quite insoluble, and sulphate of ammonia of the highest solubility. The building up of the calcareous skeleton becomes, upon this hypothesis, of exceeding simplicity. The surrounding element yields at once to the exhaling carbonate of ammonia the framework of stone.

With this view, there is no difficulty in finding a supply of carbonate of lime for the vast masses of coral. The sulphate of lime, decomposed to furnish the carbonate, is perpetually renewed through rivers from the continents and islands.

The following inferences are legitimately deducible from this view:

1st. Corals would in general, other circumstances being equal, be more likely to flourish near the mouths of freshwater streams.

2d. They would flourish in oceanic currents of moderate depth, at no great distance from the mouths of freshwater streams.

3d. They would soon die in bodies of salt water wholly cut off from the ocean.

4th. They might flourish to some extent in waters accessible to the sea only at high tide.

The second inference expresses the condition of the Florida reefs with regard to the waters of the Mississippi. DANA, in his Article vi on Coral Reefs and Islands*, remarks : " It would almost seem as if corals grew best near freshwater streams." In the same paper, the author remarks of the influence of tidal or local marine currents upon the production of harbors about coral-bound islands. In Part II of the same paper†, he states that " where there is an open chan-

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* Silliman & Dana, Am. Jour. Science, 2d Series, No. 37, p. 34 to 41.

† Ib. id., No. 34, p. 36.

nel, or the tides gain access over a barrier reef, corals continue to grow, etc. At Hennake the sea is shut out except at high water, and there were consequently but few species of corals, etc. At Ahii there was a small entrance to the lagoon; and though comparatively shallow, corals were growing over a large portion."

These facts seem to me to give some consideration to the view expressed above.

It was of interest to ascertain, in the case of corals, whether the formation of new coral without was attended with absorption or partial solution in the interior, and a corresponding reduction of its specific gravity. Specimens of coral, from the centre, periphery, and midway between, of a mass of Meandrina a foot in diameter, were reduced to powder, washed with hot water until the chloride of sodium was all removed, and their specific gravity ascertained by Storer. The average of three specimens from the centre, three from the middle, and two from the periphery, gave the following specific gravities:

Centre.	Middle.	Periphery.
2,695	2,749	2,785

These results so far support the affirmative of the suggestion above, as to make a repetition of the determinations desirable.

The chief conclusions to which the above research has conducted, are:

I. That the submerged or oolitic rock has been solidified by the infiltration of finely powdered (not dissolved) carbonate of lime, increasing the points of contact; and the introduction of a small quantity of animal mucilaginous matter, serving the same purpose as the carbonate of lime, that of increasing the cohesive attraction.

II. That the surface rock has been solidified by having, in addition to the above agencies, the aid of a series of chemical decompositions and recompositions resulting in the formation of a cement.

And I may add that it lends support to the suggestion,

III. That the carbonate of lime of corals is derived from the sulphate in sea-water, by double decomposition with the carbonate of ammonia exhaled from the living animal.

I examined, also, all the species of coral at my command, without finding a trace of alumina in any of them. The hydrochloric acid solution of the coral was precipitated with ammonia. The washed

precipitate was digested for several hours with potassa (previously tested for alumina), and filtered. The filtrate was then neutralized with hydrochloric acid, and ammonia added. After standing for several hours, there appeared filaments which were soluble neither in potassa nor nitric acid, and which, examined with a microscope, proved to be paper : they had been derived from the filter. Beside these, there was no precipitate. The quantities employed were, in several instances, from a quarter to half a pound of material.

There were examined,

Millepora alicornis;
Meandrina labyrinthica, two specimens;
Manicina palmata;
Mycidia areolata;
Astræa microcosmos, two specimens;
Rock subaërial, and Rock submarine, numerous specimens.

NOTE. In the preceding analyses, the lime due to the water, forming a hydrate, is as 9 : 28 = 2,17 : 6,75. Deducting this from the total, 51,17 — 6,75 = 44,42, we have the lime combined with carbonic acid. Calculation required 34,90CO_2 : determination gave 34,24.

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8. ON THE ANALYSIS OF URINARY CALCULI. By Professor J. LAWRENCE SMITH.

[*Not received.*]

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9. ANALYSES OF OBSERVATIONS OF THE SOILS OF PIKE COUNTY, SCIOTO VALLEY, OHIO. By D. A. WELLS, of Cambridge.

[*Not received.*]

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10. On the Homologies of the Alcohols and their Derivatives. By T. S. Hunt, of the Geological Commission of Canada.

Mr. Hunt commenced by defining the nature of the terms *homologue* and *homologous*, as applied to the constitution of organic bodies, and proceeded to lay down several distinctions which the present state of the science rendered necessary.

Homologues may be either *formal* or *functional*. Formal homologues are those which may be represented by the same general formula, but do not by their transformations afford any evidence of similarity in their constitution : they may be isomeres, as glycocoll and nitrous ether, or the functional homologues of one and the other of these.

Functional homologues undergo similar transformations, and afford products themselves homologous ; e. g. the alcohols $(CH_2)nO$, which yield hydrocarbons $(CH_2)n$ and acids $(CH_2)nO$. They are *special*, when, of a series, one of the products of the decomposition is always the same, while the others are homologous ; as, for example, the acetic ethers, all of which yield on the one hand acetic acid, and on the other homologous alcohols.

When neither the one or the other of the factors is specifically the same, but both of the products of one member of the series are homologues of those of another member, the homology is still functional, but may be said to be a general functional homology ; e. g. the ethers of different alcohols with different acids of the series $(CH_2)nO_2$. General homologues always include special homologues.

Special functional homologues may be isomeres, as in ethers or amids of bibasic acids ; e. g. methylic ether and vinic alcohol, the binamids of methamine with a binamid of ammonia and ethamine.

In order to constitute a special homologue, it is further necessary that the factor which remains specifically the same, be the generic or typical one ; for the two, as will be shown, are identical.

In another essay, I have shown that the body containing hydrogen replaceable by a metal (saline hydrogen) is to be invariably received as the parent substance : hence water is the type of all acids, oxyds, alcohols, etc. ; and acids are at once the generic type and parent of their derived ethers, amids, etc.

The alcohols differ from water by $(CH_2)n$, and water is their

functional homologue. The ethers are also special functional homologues of their respective acids. When an acid reacts upon an alcohol, water and an ether are formed, in both of which, as well as in the original substance, the aqueous type is preserved. Hence, as in all double decompositions, the reacting species regenerate themselves generically : the species change, but the genera are preserved. Ampère's theory of the double decomposition between HH and ClCl, producing HCl, HCl, is one which represents every case of double decomposition.

Mr. Hunt deduced from this some considerations as to the nature of chemical activity and molecular forces, and then proceeded to apply to the alcoholic derivatives the principles above defined. He entered at length into the theory of the formation of the organic alkaloids, and of the action of reducing agents upon nitric compounds, and explained the theory of the formation of the compound ammonias. The new alkaloid of M. Hofmann, containing the elements of ammonia and 4 equivalents of alcohol, was shown to be hydrated oxyd of tetraethammonium, and to be in effect a special functional homologue of hydrate of potassa (KH)O, to which its properties bear a close resemblance.

The author terminated his memoir by a sketch of all the different genera of compounds which have been derived from the transformation of the alcohols.

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11. Experiments on the Volatilization of Phosphoric Acid in Acid Solutions. By Orange Judd, Yale Laboratory.

In the American Journal of Science of May last, Mr. Bunce published some experiments, which show that phosphoric acid volatilizes when evaporated in an acid solution, and that in some cases the loss of PO_5 from this source is very great.

In analysing the ash of plants, I have attempted to avoid any loss arising from this source, by precipitating the PO_5 before evaporating to separate silica; and afterwards separating the silica carried down with phosphate of iron, and subtracting it before calculating the PO_5. This process, though troublesome, must be preferable where there is any liability to loss of PO_5 by evaporating in an acid solution.

After some experiments of this kind, I prepared an artificial ash,

by thoroughly mixing carbonate of potash, chloride of potassium, phosphate of soda, soda, lime, sulphate of magnesia, and a fused mass of silica and carbonate of soda. Ten determinations of the PO_5 in the mixture were then carefully made, upon nearly equal quantities. The 1st, 3d, 5th, 7th and 9th trials were made by evaporating first to separate the silica (the 3d and 7th being evaporated more rapidly than the others); and the 2d, 4th, 6th, 8th and 10th determinations were made without first separating the silica. The results were as follows:

Silica first separated.	Percentage of PO_5.	PO_5 first precipitated.	Percentage of PO_5.
(1)	17,38	(2)	17,92
(3)	17,16	(4)	17,81
(5)	17,30	(6)	18,33
(7)	17,24	(8)	17,47
(9)	17,28	(10)	18,10
Mean	17,27.	Mean	17,97.

Difference in favor of new process, 0,70 per cent.

Several grammes of the same mixture were then dissolved in HCl in a glass retort, and distilled nearly to dryness. The distillate gave the reaction for PO_5 with molybdate of ammonia; but yielded a very small precipitate with $MgO\,SO_3$, after standing 24 hours.

These experiments, together with those made by Mr. Bunce, indicate that, combined with some bases, this acid volatilizes rapidly, while with others it does not volatilize to any great degree.

In ashes and soils similar in composition to the above mixture, there would be no great loss in the ordinary evaporation for silica; while in those in which soda predominates, a loss of at least several per cent of PO_5 would be expected, unless some base were present which would fix the PO_5.

These experiments only add to the feeling among practical chemists, that there is comparatively little dependence to be placed upon the present methods of determining this acid; and it is sincerely to be hoped that some more satisfactory method will soon be discovered for its isolation and determination.

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12. Note on Ammonia in the Atmosphere. By Prof. E. N. Horsford, of Cambridge.

[*Not received.*]

13. Analysis of the Ash of a Cotton Stalk. By Orange Judd, Yale Analytical Laboratory.

While comparing the composition of the ash of several agricultural products with that of the soils upon which they are grown, I was surprised to find an almost entire absence of published analyses of the ash of one of the principal staple products of our country, viz. the Cotton Plant.

I know of no recorded analyses of the *ash* of this plant, except one of the *wool* and another of the *seed*, made by Prof. W. Shepherd, and published in the second volume of the Patent Office Reports for 1849 – 50. Happening to have in my possession a stalk of the variety called *Mexican Green*, I made some experiments with the ash in June and July. This stalk I pulled on the 19th of December last, after the close of the "picking season," from a plantation in Louisiana opposite to Bruinsburg on the Mississippi river. The stalk was of medium size, and bore about 450 buds. The lower branches and root were not preserved. After several days drying in the water bath, three determinations of amount of ash gave,

Near root.	Middle.	Section of upper branches.
3,001 per cent.	3,002 per cent.	3,419 per cent.

Mean percentage of ash, 3,141.

The ash was freely soluble in HCl; and a qualitative analysis showed the presence of $KO+CaO+MgO+SiO_3+CO_2+PO_5+SO_3+Cl$, and the absence of any trace of NaO or Fe. To avoid a possibility of loss of PO_5 by evaporation, at the suggestion of Prof. Norton, I first precipitated the PO_5 with oxide of iron and ammonia, and afterwards separated and subtracted the silica carried down with the NH_4O precipitate.

As it has been proved that under certain circumstances PO_5 evaporates (see paper by Mr. Bunce in American Journal of Science of May 1851), it is probably best, generally, to adopt the above method in determining PO_5 when silica is present, until it is satisfactorily ascertained under what circumstances PO_5 volatilizes, and when it does not.

Two or more determinations of each element were made, with the following results:

	I.	II.	III.	Mean.
Potash	23,46	24,72	....	24,09
Lime	19,92	19,72	....	19,82
Magnesia	3,07	3,01	....	3,04
Chlorine........	0,57	0,49	....	0,53
Phosphoric acid,	28,89	27,98	....	28,44
Sulphuric acid ..	2,79	2,89	2,96	2,88
Silica	2,49	2,80	2,68	2,64
Carbonic acid ...	13,98	14,66	....	14,32
Charcoal and sand,	3,71	3,70	3,89	3,76
				99,52

Rejecting the charcoal, sand and carbonic acid, and calculating upon 100 parts, we have

Potash	29,58
Lime	24,34
Magnesia	3,73
Chlorine........	0,65
Phosphoric acid .	34,92
Sulphuric acid ..	3,54
Silica	3,24
	100,00

The easy fusibility of the ash prevented a more complete separation of charcoal and sand. All error from this source, however, was avoided by thoroughly mixing and grinding the whole ash together in a mortar.

The above analysis of one plant, from a single locality, can only give an approximate view of the characteristic composition of the ash of this important staple product. A complete determination will require a large number of experiments upon the different varieties, from several localities; as well as separate experiments upon the stalk, leaves, seed and wool, and at different periods of growth.

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14. Analysis of the Brain of the Ox. By Dr. D. Breed, of New-York.

Water in the cerebrum....	78,70
Water in the cerebellum ..	78,36
Ash in fresh substance	1,44
Ash in dry substance......	6,88

CONSTITUENTS OF THE ASH.

Pyrophosphate of potassa	48,14
— of soda	20,32
— of lime	5,69
— of magnesia,	2,61
— of iron	0,61
Chloride of sodium	5,86
Free phosphoric acid	14,98
Silicic acid	0,84
Sulphur	a trace
	99,05

The preceding results are from a brain burned without adding any substance; but a second brain was burned after being rubbed to an emulsion with baryta (BaO), yet the latter gave no more phosphoric acid than the former. In the last combustions of brain, lime has been used with most gratifying success; but there is doubt whether any base is necessarily added, except to keep the ash from fusing into a mass.

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15. Analysis of the Cucumber *(Cucumis sativus)*. By J. H. Salisbury, M. D., of Albany.

Two varieties only were examined: the *Early Long Prickly*, and the *White Spine*. They were in a fit condition for table use. Length of the fruit of the Early Long Prickly, 6½ inches; diameter, 1¾ inches. Length of the fruit of the White Spine, 5 inches; diameter, 1½ inches.

PERCENTAGE OF WATER, DRY MATTER, AND ASH OF FRUIT.

	Long Prickly.	White Spine.
Water	96,364	96,605
Dry matter	3,636	3,395
Ash	0,362	0,382
Ash in dry matter,	9,955	11,252

In the fruit of this plant, we see a remarkable instance of the extent to which water may exist in a vegetable: but about 3½ lbs. of dry matter is contained in 100 lbs. of the fresh fruit. One ton would contain but about 70 lbs. of dry matter: hence one ton of fresh cucumber fruit has less dry matter than 1½ bushels of wheat. One ton of the fruit of the Long Prickly contains of inorganic matter, 7,24 lbs.; one ton of the White Spine, 7,44 lbs. 27624 lbs. of the fresh fruit of the Long Prickly variety, and 26178 lbs. of the fresh fruit of the White Spine variety, give each 100 lbs. of inorganic matter. These 100 lbs. of inorganic matter, in each case, are constituted as follows:

	100 lbs. ASH OF Long Prickly;	White Spine.
Carbonic acid	13,25	13,26
Silicic acid	0,70	0,80
Phosphoric acid	18,90	17,26
Phosphate of iron ...	3,10	2,74
Lime	4,30	4,40
Magnesia	0,20	0,34
Potash	23,20	23,30
Soda...............	33,75	33,86
Chlorine	1,10	1,46
Sulphuric acid	0,90	1,40
Organic matter	trace	trace
	99,40	98,42

The inorganic matter, as is seen, is composed mostly of phosphoric acid, potash and soda : this would indicate that ashes, bones, and common salt would be, properly combined, an excellent inorganic food for them.

PROXIMATE ORGANIC ANALYSIS.

	100 lbs. of Long Prickly.		100 lbs. of White Spine.	
	Fresh fruit.	Dry fruit.	Fresh fruit.	Dry fruit.
Albumen	0,356	7,778	0,347	7,699
Casein	0,040	0,872	0,062	1,357
Dextrine	0,354	7,736	0,264	5,894
Sugar and extract,	2,826	67,756	3,036	0,065
Starch	0,002	0,044	0,003	66,624
Chlorophyll	0,006	0,132	0,005	0,108
Fat, wax and resin,	0,031	0,682	0,029	0,629
Fibre	0,961	21,000	0,826	17,924
Dry matter	4,576	100,000	4,572	100,000
Water	95,354		95,349	
	99,930		99,921	

Besides the above bodies, the cucumber contains a small quantity of malic acid, and a still smaller percentage of citric.

One ton of the fresh fruit of the Long Prickly variety contains of sugar, 56,52 lbs.; of albumen and casein, 7,8 lbs.; of dextrine and starch, 7,12 lbs. One ton of the fresh fruit of the White Spine variety contains of albumen and casein, 8,18 lbs.; of dextrine and starch, 5,34 lbs.; of sugar, 60,72 lbs. By far the greater portion of the dry matter of the cucumber is sugar.

ULTIMATE ORGANIC ANALYSIS OF FRUIT.

	Long Prickly.	White Spine.
Nitrogen	1,236	1,301
Oxygen	41,806	41,832
Carbon......	40,984	40,467
Hydrogen ...	6,870	6,723

16. On Phosphoric Acid in Normal Human Urine. By Dr. D. Breed, of New-York.

Notwithstanding the importance of all knowledge appertaining to physiology and pathology, neither chemist nor physician has heretofore made any considerable research in relation to phosphoric acid in urine; but the accurate and expeditious method for the determination of phosphoric acid proposed by Prof. Liebig, forbids our remaining longer in ignorance of facts which may be of great value in the treatment of disease. This method consists simply of the trituration of urine with a solution of the perchloride of iron, until the filtrate from the mixture gives the well known blue reaction with ferrocyanide of potassium. It is based upon the fact that either a neutral or acetic acid solution of phosphoric acid gives with perchloride of iron an insoluble precipitate, whilst the peroxide of iron is readily dissolved by acetic acid. If a solution of phosphoric acid, containing acetate of soda, be treated with perchloride of iron, we have the following reaction:

$$PO_5 + 3(NaO\,A) + Fe_2\,Cl_3 = Fe_2\,O_3\,PO_5 + 3(NaCl) + 3A.$$

The solution of perchloride of iron is most conveniently made by dissolving in nitro-hydrochloric acid 15656 grammes of iron, and evaporating to dryness to expel the excess of acid; then adding nitric acid, and again evaporating to prevent the existence of protochloride, and afterwards redissolving the product in 2000 cc. of water. Every cc. of this solution will precipitate ten milligrammes of phosphoric acid.

Instead of the above, a solution of the perchloride of iron of unknown strength may be used, and the iron be determined; or the strength of the solution of iron may be found by trituration with a solution of phosphoric acid of known strength. In all the above methods, the solution of iron must be free from protochloride.

If the urine of which we would determine the phosphoric acid has become alkaline by the decomposition of urea, some of the phosphoric acid may have been precipitated with lime or magnesia, and it may be necessary to dissolve the precipitate by a few drops of hydrochloric acid. The urine is measured, and shaken well: with a pipet, 100 cc. or more are drawn off into a beaker glass; acetate of soda (much, if hydrochloric acid has been used) and free acetic

acid are added. The urine is then treated with a solution of perchloride of iron from a burette; frequently testing, until the phosphoric acid is saturated, and we have a drop of iron in excess. To test for iron in excess, lay a filter on a white porcelain plate, or on a glass supported by white paper, and moisten it slightly with ferrocyanide of potassium : with a glass rod bearing a drop of urine, press a double filter upon the paper containing the ferrocyanide of potassium; and if there be an excess of iron, in three or four seconds we have the blue reaction. Noting the quantity of the solution of perchloride of iron used, we proceed in like manner with two other portions of urine : if the results agree, we then calculate the amount of the solution of iron required for all the urine, from what was found necessary in 100 cc.; and multiplying the number of cc. required in all by the amount of phosphoric acid known to correspond to one cc. of the iron, we have the amount of phosphoric acid in the whole urine. By this method, a physician who has but little leisure may make each day many determinations of phosphoric acid.

The amount of phosphoric acid from an individual in health, and partaking of a uniform diet, is nearly uniform; but experiments now being made show that diet, disease, and medicines vary the amount of phosphoric acid from the normal standard.

These considerations, whilst they show the necessity of a more extensive and varied course of experiments, indicate that farther research may lead to discoveries in regard to the constituents of urine, which may be of the highest importance in therapeutics. Prout, Bright, and others have but opened a field of labor and discovery, upon which the chemist is now earnestly called to enter. Numerous statistics in regard to normal urine from individuals of different temperaments and modes of living, are first necessary.

Pathologists must critically observe the effects of various maladies upon the urine*; and then we shall have data whence to reason, and possibly to discover, not only the causes and cure of calculus without the use of the surgeon's knife, but light also may be thrown upon the treatment of many other diseases which now baffle the skill of the physician.

* Dr. Breed has discovered phosphoric acid in a fluid which Prof. Vogel of Giessen obtained by the operation of paracentesis abdominis. From 3105 cc. of the fluid, 4,648 grammes of phosphoric acid were procured. In the urine of this patient during twenty-four hours, only 1,2 grammes of phosphoric acid were found.

Determination of Phosphoric Acid in Urine.

The following results were obtained from the urine of four persons — mostly from that of one :

Urine of 24 hours.	Phosphoric acid.	Urine of 24 hours.	Phosphoric acid.
1645 cc	2,118 grammes.	1607 cc	9,831 grammes.
1150 "	2,909 "	2075 "	4,336 "
1690 "	3,454 "	1203 "	2,719 "
1675 "	2,611 "	1640 "	2,837 "
1462 "	3,647 "	2655 "	6,051 "
1462 "	3,647 "	1777 "	3,126 "
*1707 "	3,744 "	2058 "	3,888 "
*1707 "	3,744 "	1768 "	3,407 "
*1701 "	6,447 "	985 "	3,384 "
*1701 "	6,447 "	1561 "	3,941 "
*1380 "	2,862 "	1916 "	4,946 "
*1380 "	2,862 "	740 "	2,523 "

AVERAGE. 1000 cc gives 2,317 grammes phosphoric acid; 24 hours gives 1610 cc urine, and 3,732 grammes phosphoric acid.

Results from a person who drank an excess of three pints of water daily.

Urine of 24 hours.	Phosphoric acid.
2470 cc	4,288 grammes.
2407 "	4,274 "
1548 "	4,006 "
1919 "	4,344 "

AVERAGE. In 1000 cc urine, 2,027 grammes phosphoric acid;
in 24 hours, 2086 cc urine and 4,228 grammes phosphoric acid.

Results from a person who drank but half his usual amount of fluid.

Urine of 24 hours.	Phosphoric acid.
787 cc	3,807 grammes.
1220 "	4,218 "
950 "	3,904 "
997 "	4,133 "

AVERAGE. In 1000 cc of urine, 4,062 grammes phosphoric acid;
in 24 hours, 988 cc urine, 4,015 grammes phosphoric acid.

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* Average of two days.

Results from urine secreted during the waking hours, and during those of sleep.

NIGHT.

Urine.	Phosphoric acid.	Urine.	Phosphoric acid.
627 cc	1,472 grammes.	540 cc	1,033 grammes.
627 "	1,472 "	360 "	1,258 "
650 "	1,169 "	450 "	1,223 "
650 "	1,169 "	325 "	0,634 "
671 "	2,385 "	325 "	0,634 "
671 "	2,385 "	462 "	0,927 "
635 "	0,999 "	185 "	0,528 "
635 "	0,999 "	415 "	1,364 "
980 "	1,611 "		

AVERAGE. In 1000 cc urine, 2,284 grammes phosphoric acid;
in 24 hours, 1854 cc urine and 4,234 grammes phosphoric acid.

DAY.

Urine.	Phosphoric acid.	Urine.	Phosphoric acid.
835 cc	2,174 grammes.	1078 cc	2,277 grammes.
835 "	2,174 "	1236 "	2,374 "
1057 "	2,575 "	625 "	2,127 "
1058 "	2,576 "	765 "	2,367 "
1030 "	4,061 "	1103 "	2,718 "
1030 "	4,062 "	1454 "	4,019 "
745 "	1,904 "	555 "	1,796 "
745 "	1,904 "		

AVERAGE. In 1000 cc urine, 2,763 grammes phosphoric acid;
in 24 hours, 1748 cc urine and 4,831 grammes phosphoric acid.

Results from a person who drank an excess of water.

NIGHT.

Urine.	Phosphoric acid.
1050 cc	1,351 grammes.
910 "	1,496 "
230 "	0,827 "
435 "	0,934 "

AVERAGE. In 1000 cc urine, 1,755 grammes phosphoric acid.

DAY.

Urine.	Phosphoric acid.
1420 cc	2,936 grammes.
1497 "	2,781 "
1318 "	3,179 "
1484 "	3,410 "

AVERAGE. In 1000 cc urine, 2,130 grammes phosphoric acid.

Results from a person who drank very little.

NIGHT.

Urine.	Phosphoric acid.
245 cc	1,132 grammes.
377 "	1,549 "
435 "	0,983 "
382 "	1,516 "

AVERAGE. In 1000 cc of urine, 3,597 grammes phosphoric acid.

DAY.

Urine.	Phosphoric acid.
542 cc	2,764 grammes.
843 "	2,668 "
615 "	2,617 "

AVERAGE. In 1000 urine, 3,979 grammes phosphoric acid.

Results from urine secreted before and after dinner, exclusive of the hours of sleep.

BEFORE DINNER.

Urine.	Phosphoric acid.	Urine.	Phosphoric acid.
360 cc	0,995 grammes.	803 cc	1,485 grammes.
360 "	0,995 "	612 "	1,510 "
815 "	1,377 "	613 "	1,510 "
919 "	1,510 "	962 "	1,845 "
405 "	1,415 "	245 "	0,722 "
585 "	1,595 "		

AVERAGE. In 1000 cc urine, 2,239 grammes phosphoric acid;
in 24 hours, 1418 cc urine and 3,177 grammes phosphoric acid.

AFTER DINNER.

Urine.	Phosphoric acid.	Urine.	Phosphoric acid.
260 cc	0,908 grammes.	180 cc	0,772 grammes.
260 "	0,908 "	300 "	1,233 "
263 "	0,901 "	492 "	2,174 "
217 "	0,863 "	310 "	1,274 "
220 "	0,712 "		

AVERAGE. In 1000 cc urine, 3,745 grammes phosphoric acid;
in 24 hours, 1120 cc urine and 4,881 grammes phosphoric acid.

Results from a person who drank an excess of water.

BEFORE DINNER.

Urine.	Phosphoric acid.
960 cc	1,447 grammes.
1212 "	1,826 "
363 "	1,164 "
1004 "	1,738 "

AVERAGE. In 1000 cc urine, 1,743 grammes phosphoric acid.

AFTER DINNER.

Urine.	Phosphoric acid.
460 cc	1,490 grammes.
280 "	0,955 "
955 "	2,011 "
480 "	1,678 "

AVERAGE. In 1000 cc urine, 2,820 grammes phosphoric acid.

Results from a person who drank very little.

BEFORE DINNER.

Urine.	Phosphoric acid.
360 cc	1,627 grammes.
620 "	1,751 "
560 "	1,473 "
230 "	0,891 "

AVERAGE. In 1000 cc of urine, 2,344 grammes phosphoric acid.

AFTER DINNER.

Urine.	Phosphoric acid.
182 cc	1,047 grammes.
223 "	0,916 "
385 "	1,725 "

AVERAGE. In 1000 cc urine, 4,541 grammes phosphoric acid.

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17. ON THE EXISTENCE OF ORGANIC MATTER IN STALACTITES AND STALAGMITES, FORMING CRYSTALLIZED AND AMORPHOUS CRENATE OF LIME. By DAVID A. WELLS, of Cambridge.

IN the eighth chapter of Liebig's Agricultural Chemistry, edited by Playfair, we have given the result of some examinations of stalactites from caverns in Germany, and from the vaults of old castles upon the Rhine, made with the view of ascertaining the fact of the presence or absence of organic matter in these bodies, either combined or uncombined. The result may be stated in the words of the author, Prof. LIEBIG :

The stalactites from the caverns " contain no trace of vegetable matter, and no humic acid, and may be heated to redness without becoming black." In the stalactites from the vaults and cellars of old castles, he says, " we could not detect the smallest traces" of humic acid. " There could scarcely be found a more clear and convincing proof of the absence of the humic acid of chemists in common

vegetable mould." Under the term *humic acid*, Prof. Liebig undoubtedly means to include all those organic acids arising from the decomposition of vegetable matter, and which have received the names of *crenic*, *apocrenic*, *geic*, and *humic acids*.

Having been informed by Dr. A. A. Hayes, of Boston, that he had in numerous examinations arrived at results directly opposed to those of Prof. Liebig, I was induced, at his suggestion, to make an examination of a large number of stalactites and stalagmites, obtained from various localities, with reference solely to the presence or absence of organic matter in these bodies. The specimens examined were all from caverns or rock formations, and were obtained from various parts of the United States; from Trieste, Austria; from Malta, and the Sandwich Islands. In color they varied from an almost pure white, to red, yellow, and brown of different shades; and in crystalline character, from a structure resembling arragonite, to those entirely wanting in symmetrical arrangement, or mere incrustations. The specimens were dissolved in dilute hydrochloric acid; the flocculent matter separated, collected and washed; boiled with caustic potassa, carbonate of ammonia or carbonate of soda, and then tested in the usual way for crenic and apocrenic acids by acetate of copper and carbonate of ammonia. In all the varieties, with one exception, abundant flocculent organic matter was separated, which, on testing, gave evidence of crenic acid in considerable quantities, with doubtful traces of apocrenic acid. The exception alluded to was the specimen examined from Trieste, which did not afford any appreciable flocculent matter on dissolving acid. The greatest quantity of organic matter was found in stalactites of a deep yellow color, highly crystalline and uniform in character: the portions examined were perfectly homogeneous, and free from layers or intervening bands indicating different periods or changes in deposition. As the presence of iron could not be detected in the acid solution, it is inferred that the color of these yellow stalactites may be owing in great part to combined organic matter, existing as crenate of lime. In specimens like the spar ornaments from the Rock of Gibraltar, with which all are familiar, the coloring matter and delicate shading are also probably due to organic matter. Dr. Hayes informs me that he has also found organic matter in arragonite, in sufficient quantity to separate in flakes while the specimen was dissolving in acid.

From these statements, it must, I think, be inferred, contrary to

the views of Liebig, that organic matter does exist in stalactites generally, as an acid combined with the lime, and imparting to them their various colors. I would by no means call in question the accuracy of Prof. Liebig's experiments, further than that as far as my observations extend, crenic acid in the presence of lime, and combined with it, passes, like oxalates, upon heating, into carbonates, without perceptible blackening.

It may here be added, that Prof. JOHNSON, of England, describes a compound of alumina and crenic acid occurring in caves of granite on the coast of Cornwal. This mineral has received the name of *pigotite*, and is observed in places where the surface water trickles down over the granitic rocks. From this it may not be inappropriate to apply the term *crenite*, to those lime formations in which crenic acid occurs in considerable quantities.

Results similar to those announced above, have been obtained by Dr. C. T. JACKSON, as well as Dr. HAYES of Boston. Dr. J. LAWRENCE SMITH informs me that he has frequently met with crenic acid in calcareous concretions from Asia Minor, and its existence in stalactites was also announced some years since by Dr. EMMONS of Albany. My results, therefore, can be considered as verifications only of those obtained by others.

II. MINERALOGY.

18. NOTICE OF SEVERAL AMERICAN MINERALS. By Professor C. U. SHEPARD.

1. *Nickeliferous Iron Pyrites, from Kearney Ore Bed, Gouverneur, St. Lawrence County, N. Y.*

A specimen of this ore was lately sent to me by Mr. VAILL, of Litchfield (Connecticut), with an inquiry as to its character. It is in botryoidal concretions, resembling sphærosiderite in size and shape, but possessing a more distinctly marked radiating and fibrous structure. A fresh fracture exhibits a pale bronze-like color, and the usual lustre of iron pyrites.

Hardness = 5,5. Sp. gr. = 4,863.

When heated before the blowpipe, it decrepitates violently; but if first heated in a glass tube, the little fragments will afterwards remain upon the charcoal support, and fuse together into a hollow black globule, which is strongly magnetic. It is not readily attacked by either hydrochloric or nitric acid separately, but slowly yields in a mixture of the two. After precipitation by ammonia, a pale blue ammoniacal solution of oxide of nickel appears; but having made several trials on similar weights of the mineral, I satisfied myself that the proportion of nickel varied in different portions of the pyrites, and that in each case it was much below that found by Scheerer in his Eisennickelkies from Norway (See Pogg. B. lviii, s. 30, 1843). Its occurrence, however, seems to me interesting from the presence of millerite at the same locality*; besides which, the association of nickel with iron ores assumes an additional importance, from the fact of their almost constant connexion in meteorites. Other ores of nickel will probably reward a scientific examination of the Antwerp and Rossie iron ores.

2. *Leadhillite from Newberry District, S. C.*

I am indebted for a knowledge of the existence of this species of lead ore in South-Carolina to Prof. HUME, of Charleston, to whom it was sent along with a variety of minerals for examination. It is well crystallized in apparently regular hexagonal prisms, and six-sided, very acute pyramids. Color pale greenish or yellowish white. When reduced to powder, it is rapidly dissolved in nitric acid with effervescence, except about ¼th its weight of sulphate of lead, which is left unattacked by the acid. Judging from the character of the specimens sent, this rare mineral must be abundant at the locality.

3. *Large Crystal of Native Gold from California.*

To the above mentioned gentleman (who is the State Assayer), I am also indebted for the largest crystal of native gold I have ever seen, the interest of which is likewise enhanced by its presenting a form not hitherto observed among the secondaries of the species. It measures ⅜ths of an inch in diameter (weighs 121,1 grs.), and has the shape of the pentagonal dodecahedron. Notwithstanding it has

* A greenish grey kaolin, resembling in color pimelite in a slight degree, occurs with the millerite.

formed a loose mass in the diluvium of the gold region, and been subjected to considerable friction, it still presents distinct traces of the raised edges so characteristic of the native gold crystals of California, and which militates against the idea that these forms were produced in moulds left vacant from the decomposition of iron pyrites.

4. *Large Crystal of Quartz found at Waterbury, Vt.*

For a knowledge of this gigantic crystal, I am indebted to Mr. M. M. Carleton, A. B., lately a student in Amherst College. It was found as a boulder in the field, and, from the marks upon it, seems to have suffered from the action of drift. Its present weight is 175 pounds. It is a regular six-sided prism, rather more than two feet in length; one end having been broken off, and from it rather more than half a bushel of fragments detached by the curious who have inspected it : its original weight, therefore, must have been at least 200 lbs. The width of two of its opposite broadest lateral planes is 14 inches; that of the others, from 8 to 9. Two of its pyramidal planes are still preserved. The crystal is said to be quite clear in the interior.

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19. On Chalcodite, a new Mineral Species. By Professor C. U. Shepard.

Primary Form. Rhomboid : dimensions unknown.

Compound varieties. In globules and stellular groups. Composition columnar; the individuals flat, and slightly coherent. Surface of the globules drusy in those of the largest size; rarely seen to be made up from the crossing of lenticular crystals, as in certain varieties of specular iron (with which species, under this form, the chalcodite is sometimes associated). It also occurs massive, in minute thin flakes, or finely granular, in the form of coatings, disseminated through specular iron, quartz and calcite.

Cleavage. The columnar individuals possess one very distinct cleavage, upon which the lustre is pearly to semi-metallic.

Color dark blackish green (the globular concretions velvety), when imbedded in calcite; but some shade of brown, usually coppery or bronze, when on quartz and specular iron. The prevailing aspect is that of mosaic gold. Streak corresponding to the color, though paler : translucent.

Sectile; the laminæ very flexible. Hardness = 1,0.

Heated before the blowpipe in a closed tube, it emits water plentifully, and the darker colored varieties change slightly to brown. On charcoal, it fuses into a black glass, which is magnetic. It disappears with rapid effervescence in borax, affording a glass deeply stained with iron.

The mineral is not acted upon sensibly by cold acids; but dissolves easily in hot hydrochloric acid, with separation of silica. Ammonia produces a precipitate chiefly of protoxide of iron; but after treatment with nitric acid, the same precipitant yields sesquioxide of iron; and the liquid, being cleaned, gives traces of lime, with a decided precipitate of magnesia, on the addition of the usual tests for these earths. It was not convenient to examine the precipitate by ammonia, for alumina; but presuming upon its absence, from the easy decomposition of the mineral by hydrochloric acid, the mineral will most probably prove on analysis to be a hydrated silicate of protoxide of iron (possibly mixed with the peroxide in the brown colored variety) and magnesia.

In natural history properties, the species will stand near to talc; from which, however, many of the foregoing properties tend to separate it, and from which it will doubtless be seen to diverge still further, when enough of it can be had to allow of the correct determination of its specific gravity.

It is named *chalcodite*, from χαλκώδης (like brass), from its prevailing pyritic or bronze-like color and lustre.

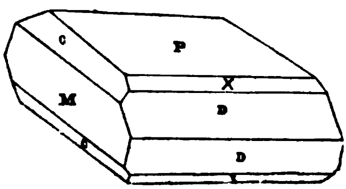
This mineral was made known during the geological survey of the State of New-York, and was referred to *cacoxene* by Dr. Lewis C. Beck in the notice of the geological survey of the State of New-York presented to the legislature January 24th, 1840*, and more particularly described by the same writer in his extended report on the mineralogy of the State, 1842, p. 402, where it is mentioned as occurring "in minute radiated tufts which somewhat resemble iron pyrites, the lustre of the blades being silky or approaching to metallic;" although it is possible the chalcodite is here confounded with the millerite, which is also found at the same locality.

It occurs at the Sterling iron mine in the town of Antwerp, Jefferson county, accompanying specular iron ore.

* American Journal of Science, Vol. xl, p. 76.

20. On the Triplite (Alluandite?) of Norwich, Mass. By Prof. C. U. Shepard.

In a paper by Prof. Dana (American Journal of Science, Vol. xi, new series, p. 100), on the crystalline form of this ambiguous mineral (of which he observes "many more crystals must be examined, before their character is fully understood"), he considers the primary form a right prism, in which M of the annexed figure is P, and P is d.



M on T....	96°	00′	com. goniometer.
P on M....	90	00	
M on d....	93	00	
—— c	108	00	
P on c	140	15	
—— x	138	15	
—— d	119	00	
x ——	167	00	
d on d	130	45	to 131° 15′.

Under this view of the form, he observes that "in many of the crystals the terminal plane is oblique to the lateral; and this obliquity, although an irregularity, is not attended with any distortion of the adjoining plane T (c of the above figure), the upper and lower edges of this plane being parallel notwithstanding the varying inclination of P."

By considering the primary form to be a right oblique-angled prism, the supposed irregularity disappears, and the occurring faces result from symmetrical modifications of the assumed primary.

The angles above given were taken (from crystals kindly supplied by Mr. Hartwell) by means of the reflecting goniometer; but it should be stated that the lustre was not sufficient to afford visible images of a window-bar in the faces, as in the ordinary and most exact use of this instrument. The results may nevertheless be regarded as very close approximations to the truth; since, in each angle given, they are the mean of numerous not widely differing observations.

The physical quality of the faces is similar: d is perhaps rather brighter than the rest. The lustre of the others is in the following order, the brightest being mentioned first: c, P, M, x. No striæ visible on the crystals.

Cleavage most distinct parallel with M : traces of T.

Hardness = 5,0. Sp. gr. = 3,27.

Color blackish brown when weathered; but within yellowish green, sometimes bluish. Streak dark reddish brown : translucent to opake.

Fusible before the blowpipe, with much effervescence, into a shining hollow globule not attractable by the magnet. With borax, it dissolves with effervescence into a glass colored by iron. Soluble with facility in hydrochloric acid, from which ammonia throws down an abundant red brown precipitate of the oxides of iron and manganese; after the subsidence of which, the clear liquid gives, with oxalate of ammonia, the test for lime.

Mr. Craw's analysis (volume of American Journal above quoted, p. 99) seems to connect this mineral with the alluandite of Damour, rather than with triphylline (though it is probable that both of these may ultimately be referred to the species triplite). For if we suppose the manganese to have become peroxidized by weathering (and the first specimens obtained were particularly exposed to this change*), and the substitution of lithia and lime (4,21 p. c. together) to have transpired for the 5,47 of soda in Damour's analysis of alluandite, the identity is nearly complete. The agreement in physical qualities between the two minerals appears to render their coalescence still farther probable.

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21. On the Isomorphism of the Chemical Compounds comprised under the Mineral Species Tourmaline. By Prof. James D. Dana.

Many instances of isomorphism among compounds not homologous in composition, or of different proportions in the elements, have been recently pointed out by Rose, Scheerer, Rammelsberg, Hermann, etc., besides others by myself in a memoir published during the past year.

The principle, brought out by M. Kopp, that in ordinary cases of isomorphism, isomorphous substances are alike in atomic volume,

* The specific gravity of Mr. Craw's specimen was only 2,876.

has been applied to these seemingly anomalous cases. In my own memoir alluded to, I suggested that if the atomic volume, as calculated in the usual manner (by dividing the atomic weight of the compound by the specific gravity), be divided by the number of atoms in the compound, we have a perfect coincidence with Kopp's law, and a satisfactory explanation of these isomorphisms.

In the division proposed, all compounds are reduced to an equivalent unit : thus, for FeO, I divided by 2, as if the compound consisted of $Fe_{\frac{1}{2}}O_{\frac{1}{2}}$, which is true of its composition, the sum of the fractions being a unit. So for Fe^2O^3, I divided the atomic volume found by 5, as if the compound consisted of $Fe_{\frac{2}{5}}O_{\frac{3}{5}}$; this accurately representing the composition, and the sum of the whole as before being a unit. These explanations apply to all other cases.

Rammelsberg, in attempting to find a correspondence with Kopp's law, deduces the principle that in these isomorphous compounds the atomic volumes have a simple multiple relation. He compares only the ordinary atomic volumes, calculated in the usual way, and thus arrives at simple ratios between the resulting amounts. Thus while Rammelsberg finds the atomic volumes unequal, and related only by certain ratios, the view I have proposed arrives at actual equality. It is of some interest to inquire which method is most satisfactory, or most correct. To test this, I take a single case presented by Rammelsberg, that of *tourmaline*, the varieties of which have been made the subject of profound chemical research by Rammelsberg*.

This distinguished chemist, after numerous analyses, has made out *five* chemical groups in this species, having the following formulas :

	Formulas of Tourmaline.	Atomic Volume.
I.	$\dot{R}^3(\dddot{Si}^2, \dddot{B}^2) + 3\,\dddot{\mathit{R}}(\dddot{Si}, \dddot{B})$,	1808 ;
II.	$\dot{R}^3(\dddot{Si}^2, \dddot{B}^2) + 4\,\dddot{\mathit{R}}(\dddot{Si}, \dddot{B})$,	2217 ;
III.	$\dot{R}^3(\dddot{Si}^2, \dddot{B}^2) + 6\,\dddot{\mathit{R}}(\dddot{Si}, \dddot{B})$,	3013 ;
IV.	$\dot{R}(\dddot{Si}, \dddot{B}) + 3\,\dddot{\mathit{R}}(\dddot{Si}, \dddot{B})$,	1464 ;
V.	$\dot{R}(\dddot{Si}, \dddot{B}) + 4\,\dddot{\mathit{R}}(\dddot{Si}, \dddot{B})$,	1850.

The corresponding atomic volumes, as deduced by him, are given in the third column.

* Poggendorff's Annalen, lxxxi, 31.

Rammelsberg then observes that the numbers 1464, 1808, 1850, 2217, 3013, have the relation (writing 1,26 as a correction of his 1,25) 1 : 1,24 : 1,26 : 1,51 : 2,06;

which is closely 1 : $1\frac{1}{4}$: $1\frac{1}{2}$: 2,

or 4 : 5 : 6 : 8.

But let us now calculate the atomic volume in the manner I have proposed; that is, by dividing each of the above amounts by the number of atoms or molecules in the corresponding compounds. The number of atoms is found to be, for Formula I, 41*; II, 50; III, 68; IV, 33; V, 42.

Then

$$1808 \div 41 = 44,1;$$
$$2217 \div 50 = 44,34;$$
$$3013 \div 68 = 44,31;$$
$$1464 \div 33 = 44,36;$$
$$1850 \div 42 = 44,05.$$

The results thus arrived at for the compounds are *equal atomic volumes* throughout, viz. 44,1, 44,34, 44,31, 44,36, 44,05; certainly a remarkable coincidence in value, and a sufficient explanation of the isomorphism.

Let us now look further at the numbers of atoms, and see what follows from the preceding facts. The ratios which these numbers have to one another, 33 : 41 : 42 : 50 : 68, are as follows:

1 : 1,24 : 1,27 : 1,51 : 2,06;

and these ratios, as is seen, are actually coincident with the atomic volume ratio obtained by Rammelsberg. *Hence the ratios* 4 : 5 : 6 : 8, which this chemist deduces, *are in fact only the ratios of the number of molecules in the compounds*, 33 : 41 : 50 : 68; a ratio more exactly expressed by these numbers themselves. They cannot therefore be considered the atomic volume ratio.

In the method which I have proposed, the principle holds true, and gives uniform results, where the compounds compared consist of like elements. Thus anhydrous silicates are directly comparable with anhydrous silicates; but in comparing sulphur and oxygen compounds, another principle probably modifies the result.

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* In the first formula, $\dot{R}^3$ contains 6 molecules; $\dddot{Si}^2$, 8; 3 $\dddot{\bar{R}}$, 15; 3 $\dddot{Si}$, 12: making in all 41.

This point requires more investigation than I have been able to give it. The case of the varieties of tourmaline, however, is one in which no modification of this kind is to be considered. This remark is true also of the felspars, the varieties of scapolite, and numerous other cases of this kind brought forward in a former memoir.

I observe, in conclusion, that although the isomorphism of certain minerals alone is here considered, the principle involved has a much wider authority. It is one of the most fundamental in chemistry. Minerals are chemical compounds no less than the salt formed by art: their origin is due to no peculiar laws, different from those that preside over combinations in the laboratory. The profoundest nature of molecules is at the basis not only of all cases of isomorphism, but of all that relates to crystallization or structure in inorganic nature.

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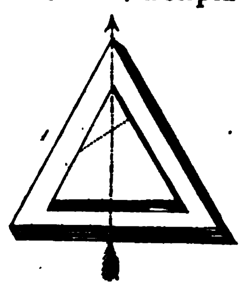
22. Optical and Blowpipe Examination of the supposed Chlorite of Chester County, Pa. By W. P. Blake.

In September, 1850, Prof. B. Silliman junior handed me a specimen of a beautiful green foliated mineral for optical examination: it was unexpectedly found to be biaxial; but as the locality of the specimen was not known, no further examination than the measurement of the angles was made at that time. In May of this year, I received from Prof. J. D. Dana specimens of the hitherto supposed chlorite of Chester county (Pennsylvania), which I examined by polarized light, and obtained results so similar to those obtained with the specimen first referred to, as to leave no doubt of its being from the same locality*.

The mineral occurs three miles south of West-Chester, in serpentine, associated with magnesite; and is found in plates of irregular outline, sometimes three inches broad, and in triangular plates and tabular masses, one of which is represented in the annexed figure. These plates are equilateral triangles; and they much resemble the triangular cleavage specimens of the micas from Greenwood furnace and Monroe, N. Y. The cleavage is



* Prof. Dana received his specimen from Thomas F. Seal, of Philadelphia.

perfect, parallel with the broad faces of these crystals; but is not so perfect as in mica, and the laminæ are more brittle. The laminæ are flexible and elastic, but less elastic than mica. Color, beautiful emerald green. Hardness of cleavage surface, 2 to 2,25, scale of Mohs. Specific gravity 2,714, which is perhaps too low, as no specimen could be obtained perfectly free from air.

Optically it is biaxial, with a high angle, and the following are the results obtained:

Specimen *a*, examined in September, plate one decimetre long and six centimetres broad, with an irregular outline.
Specimen *b*, a triangular plate measuring one and one-fourth inches along each side, examined in May.
Apparent angle between the optic axes in *a*, 84° 30′ mean of nine measurements.
Apparent angle in *b*, 85° 59′ mean of five measurements.

The plane of the axes is perpendicular to the cleavage surface, and at right angles with the base of the triangle, as indicated by the arrow in the figure. I was also able to obtain evidences of optic axes in the angle of the plate opposite to the base, and found them to have an equal inclination with *b*; and the plane of these axes was found to form an angle of about 60° with the plane of the others, or to be at right angles with one of the sides of the triangle (which is as near the angle as could be determined by marking the direction upon the plate, and subsequent measurement by goniometer and protractor). This peculiar relation of two systems of optic axes had been noticed in *a* also, and there is probably a line of composition in most of the crystals from the locality. The position of this line is represented by the shorter dotted line in the figure.

Another interesting peculiarity is, that the optic axes are *not equally inclined to the cleavage surface*, or to a line perpendicular to it (the "normal" of Biot). The inclinations to the normal were measured; but as the instrument had not been adapted to this mode of measurement, the angles given can be regarded only as approximations, and are here given merely to show the existing inequality of the inclinations.

Specimen *a* gave the angles 50° and 34°;
Specimen *b*, ... 58° 13′ and 27° 40′;

the greater angle being on the side of the normal adjoining the base of the plate or triangle.

From these results, the mineral must be referred to one of the

systems of crystallization having the three axes unequal; and it cannot therefore be classed with the species chlorite or ripidolite, which, according to authors, is rhombohedral or hexagonal. The Alabama chlorite was examined optically by Biot, and reported to be *uniaxial.*

It is here interesting to observe that we have this undoubtedly *clinometric* mineral with such a peculiarly high angle between the optic axes, occurring in triangular plates and masses so much resembling the micas from Monroe (New-York), whose biaxial character is so difficult of determination, and which by reason of this form have been referred by some eminent crystallographers to the rhombohedral system. The form in both cases may be considered as resulting from an acute oblique rhombic prism, by the replacement of the acute solid angles*.

Examined with the blowpipe, the mineral gives the following reactions : B.B. in the forceps, contracts and becomes opaque and white, with traces of fusion on the edges. Alone, on charcoal, same as with forceps. In an open tube, gives off water, and a white ring is formed near the assay when strongly heated. With borax in the oxydizing flame, dissolves readily with much ebullition; the glass, while hot, red and brownish, but becomes green when cold : in the reducing flame, while hot, color not so deep as in the oxydizing flame, and passes through the shades of olive green while cooling, to beautiful emerald green when cold. With phosphate of soda and ammonia in the oxydizing flame, dissolves slowly, leaving a skeleton of the fragments; glass red and yellowish while hot, fine green when cold; when much of the assay is added, the glass becomes opalescent to opaque when cooling : in the reducing flame, skeleton disappears; bead brown while hot, opalescent and green when cold. With carbonate of soda on platina foil, no reaction for manganese.

The constituents of the mineral, so far as indicated by the above reactions, are, $HO+SiO_3+CrO_3+FeO_3$. Analyses are now in progress at the Yale Analytical Laboratory.

In addition to the optical character, the mineral is shown to differ from chlorite in hardness and elasticity, and by the presence of chromium.

I propose for the species the name *Clinochlore,* in allusion to the

* Dana's Mineralogy, 1st edition, p. 264; and Am. Jour. Sci., 2d ser., xii, p. 8.

great obliquity between the optic axes, and its green color, resembling that of chlorite.

A similar mineral from Unionville, occurring in triangular and hexagonal forms, I have found to be biaxial, and probably like the above; but I have not yet succeeded in obtaining any measurements.

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23. Metamorphic Condition of a part of the large vein of Franklinite in New-Jersey. By A. C. Farrington, Esq., of Newark.

During the summer of 1848, while engaged in exploring the metalliferous veins upon what is called Mine Hill, near the Franklin Furnace, New-Jersey, my attention was arrested by the difference in structural arrangement presented by the opposite sides of the large vein of franklinite at different places along its extent. While much the largest portion of the mass appeared to consist of imperfect octahedral crystals compacted or cemented, other parts appeared like an aggregation of thin laminæ, its crystals resembling tabular spar. This latter portion was highly magnetic; and in pulverizing, I found the hammer would take up large quantities of it. Knowing that other parts of the vein did not exhibit this property, I pursued my investigations for the purpose of ascertaining how much of the ore presented this magnetic property. The result was, that it was found only where the tabular crystals prevailed, and they only where the vein was in contact with sienite or garnet; and in tracing across the vein in a right line, magnetic action was not perceptible for more than four feet. I repeated my experiments, and four feet three inches was the maximum distance that the ore was found magnetic.

I broke off fragments in a line across the vein at the distance of three inches from each other, and, after pulverizing, weighed one hundred grains from each parcel, and applied a common horseshoe magnet to them. The magnet would take up all, or nearly all of the powder from such parts as came from the side of the vein nearest the igneous rock, and gradually diminish as they receded from it. I failed in establishing any regular series or ratio for the diminution of magnetic action; but inferred from the results, that the iron of the franklinite, in the parts of the vein in contact with sienite and

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garnet, was a protoxide; while the mass of the vein was a peroxide, and intermediate for the distances examined as before stated there was a mechanical mixture of the two oxides.

In presenting these facts, an important geological question arises. Is the metamorphism of this metallic vein attributable to the agency of the intrusive rocks in contact with it; and if so, should we not infer that the igneous intrusive rock is more recent than the vein of franklinite?

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24. NOTICE OF A MAGNESIAN OPAL, FROM NEAR HARMANJICK, ASIA MINOR. By Prof. J. LAWRENCE SMITH, of the University of Louisiana.

THIS opal is one found in several places that have come under my observation in the western parts of Asia Minor, and the Island of Mytilene. It is found, along with carbonate of magnesia, in serpentine. The serpentine appears to have undergone a decomposition by water containing carbonic acid; the magnesia combining with the carbonic acid to form carbonate of magnesia, and the silica becoming converted into opal involving a small amount of magnesia. The composition of the opal was found:

Water..........	4,15
Silica	92,00
Magnesia........	3,00

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25. ON THE COLUMBITE OF HADDAM. By T. S. HUNT, of the Geological Commission of Canada.

[*Not received.*]

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26. ON THE OCTAHEDRAL PEROXIDE OF IRON. By T. S. HUNT, of the Geological Commission of Canada.

[*Not received.*]

27. On the Houghite of Prof. Shepard. By S. W. Johnson, of the Yale Analytical Laboratory.

On page 314 of the Proceedings of the Meeting of this Association held at New-Haven last year, occurs a notice of the mineral houghite by Prof. Shepard. More than two years since, I noticed specimens of this mineral from Dr. Hough, and then purposed to examine it. The specimens which were in my possession at the time of the publication of Prof. Shepard's paper, and which furnished the material for my analyses, agree in the main with his description; and it appeared improbable that any chemical species could be made from them, as they were exceedingly variable in composition as well as in appearance. It commonly occurs as small imbedded nodules, usually more or less flattened, with the interior dark gray or bluish gray and the exterior white.

Experiment and observation unite in proving that the difference in color between the external and internal portions of these "concretions" is due to difference of composition. The milk-white parts contain *carbonic acid*: the bluish-white portions do not. Further, some specimens are milk-white throughout; while others are almost entirely of a bluish tinge, and semitransparent. The specimens at my command for analysis were uniform in appearance, but were more or less opake white externally, and, without exception, were pervaded by minute grains of spinel and phlogopite. Some of them presented a portion just within the opake layer, that was transparent and homogeneous: within this, fragments and crystals of spinel predominated. These statements are necessary as a preface to the account of my chemical investigations; after detailing which, the physical properties will be again referred to.

The mineral subjected to analysis comprised fragments partially opake and partially translucent, as it was almost impossible to procure by separation a homogeneous material. After ignition, it manifests an alkaline reaction; and this, as I have since found, it also does before heating.

As stated by Prof. Shepard, it is decomposable in acids before and after ignition. A large nodule slowly dissolves even in cold acetic acid. Carbonic acid is evolved during solution, and in sufficient quantity to produce a precipitate on passing through baryta water. A residue has always occurred in my experiments, consisting in part

of insoluble minerals, spinel and phlogopite, and also in most cases of silica; and, in fact, the mineral has often afforded a well characterized jelly with acids. The acid solution gives with ammonia, in presence of chloride of ammonium, a copious white precipitate. The filtrate contains only magnesia, or occasionally a trace of potash, possibly from decomposition of phlogopite. The ammonia precipitate, as noticed by Prof. Shepard, yields alumina and a trace of iron to caustic potash, but is not entirely decomposed even by a large excess of it during protracted digestion at a boiling heat.

Following the usual routine of analysis, it was repeatedly and most carefully examined for all the salts and rare earths that can occur in such circumstances, but no evidence of their existence was obtained. It appeared to be a hydrous compound of magnesia and alumina; and upon reference to Gmelin's Handbuch, notice of such a substance was found. By three or four repeated solutions in hydrochloric acid, and precipitations by ammonia, it was completely separated into the two earths, alumina and magnesia; which, with water, completed the sum of its ingredients.

Previous to entering on the quantitative investigation of this mineral, I made inquiries of Dr. HOUGH, who resides near the locality, hoping to obtain homogeneous specimens: he could not furnish them, and the following analyses were made without expectation of perfectly accordant results.

In the analysis, carbonic acid was determined in the usual manner in a flask furnished with chloride of calcium and aspirating tubes. Water was expelled by ignition, and collected in chloride of calcium. The mineral was decomposed in hot hydrochloric acid; the whole evaporated to dryness, redissolved, and filtered. In the solution, alumina and magnesia were separated by bicarbonate of soda; the magnesia weighed as pyrophosphate; the alumina, as such, after solution and reprecipitation by carbonate of ammonia. The insoluble residue was treated with hot solution of carbonate of soda, to extract silica.

Before subjecting the mineral to analysis, a portion in small fragments, the most homogeneous that could be selected, was used for determining its specific gravity. For this purpose, after its weight had been taken, it was boiled in the water in which it was subsequently weighed, as air bubbles adhered to it very pertinaciously. The number 2,175 was obtained; but the mineral was afterward found to contain spinel.

A quantity of the mineral, after being pulverized, was placed in a Liebig's drying tube, and exposed in a current of dry air to a heat of 100° C.: it lost water for a long time. The heat was afterward raised to 175° C., and it continued to lose weight for several days. It was finally submitted to the highest temperature admissible in an oil-bath, 280° C. (536° F.); and after more than 100 hours of drying, it ceased to lose weight. As this result had not been foreseen, the original weight of the mineral was not taken: the loss was at least five per cent. In this dried portion were found

$\dddot{Al}^2$	$\dot{Mg}$	$\dot{H}$ (by diff.)	$\ddot{C}$	Insol. (spinel, &c.)	Silica.
19,743	36,292	24,223	8,458	8,264	3,020=100.

A fresh portion of the mineral, including that employed in taking the specific gravity, was carefully intermingled, pulverized, and dried over SO_3 *in vacuo*.

Two determinations of carbonic acid gave respectively 6,712 and 8,094 per cent: mean, 7,380 per cent. Another portion, the only remaining material, was ignited until it ceased to lose weight: it then contained no carbonic acid. The loss was 40,857 per cent. It was analyzed with the following results:

$\dddot{Al}^2$	$\dot{Mg}$		
30,048	55,467	Insol. (spinel and mica) 15,196	Silica, *trace*=100,711.

Several water estimations were made, varying from 33 to 41 per cent; but I attach no value to them. If we subtract 7,380 the mean result for carbonic acid, from 40,857 the total loss on ignition, we obtain 33,477 as the percentage of water. Rejecting the insoluble minerals, and calculating the remaining constituents on 100, the composition stands as follows:

$\dddot{Al}^2$	$\dot{Mg}$	$\dot{H}$	$\ddot{C}$
23,867	44,839	26,452	5,833=99,995.

Excluding, in both analyses, all the ingredients but alumina and magnesia, and reducing these to percentage relations, we find the following numbers:

	First analysis.	Second analysis.	Mean.
$\dddot{Al}^2$	35,238	35,137	35,185
$\dot{Mg}$	64,763	64,862	64,812

These accordant results indicate the existence of definite relations between these two ingredients. Division by the equivalents gives the

ratio of 4 of alumina to 19 magnesia; corresponding to alumina 35,15, magnesia 66,47. The ratio of 1 : 5 would require alumina 33,53, magnesia 66,47.

It seems useless to speculate on the constitution of houghite, without new analyses.

Quite recently, I have visited the locality in company with Dr. HOUGH. It is in the town of Rossie, and near the village of Somerville, in St. Lawrence county, N. Y. The mineral occurs disseminated through white crystalline limestone, at the summit of a slight elevation, near which occur beds of the Potsdam sandstone. Associated with houghite are dolomite of variable composition, scapolite of brown and green color, phlogopite, graphite, spinel, and a crystallized pseudomorphous(?) yellow serpentine, in which I have obtained the water and silica percentages of that mineral.

Much of the rock exhibits evidences of atmospheric action. The serpentine in the altered parts has become discolored, and so friable as to yield to the pressure of the fingers. The nodules of houghite are half exposed, easily detached from the rock, and often opake and milk-white throughout.

This altered or bleached appearance in the rock does not occur upon the uppermost surface, as far as I have observed, but along its sides and under portions pendant over a cavity; but not a nodule of houghite has been found, even in the least altered rock, that has not presented superficially in some parts a milk-white color.

Among the masses which furnished material for analyses, I found several specimens that exhibited unequivocal evidences of octahedral crystallization, one of which is here represented. Some of them are $\frac{3}{4}$ths of an inch in diameter; they are superficially grooved and contorted, their edges are rounded, and protrude beyond the planes of the faces. In some an appearance occurs, which seems as if it had been produced by a protrusion near the edge of each plane, leaving a line of depression with reëntering surfaces corresponding to the lateral edges of a perfect octahedron; while on the faces, a triangular depression occurs, bounded by the protruded edges of each plane of the crystal. In one nodule there is a gradual transition from the soft and amorphous houghite, to the hard and regularly terminated spinel. The crystals are occasionally compound.

28. On the Occurrence of Chromate of Lead in Pennsylvania, and other Mineralogical Notices. By W. P. Blake.

Mr. Blake presented a communication upon some American minerals, stating that he had observed the occurrence of chromate of lead in Pennsylvania, upon pyromorphite and carbonate of lead. He also noticed the occurrence of chalcotrichite at Perkiomen mine, Pennsylvania; sulphuret of nickel, in Lancaster county, Pennsylvania; and yenite, from O'Niel mine, Orange county, New-York.

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29. On Phosphate of Lime. By Prof. E. Emmons, of Albany.

[*Not received.*]

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30. On a New Locality of Red Sapphire, with Notices of the Associate Minerals. By W. P. Blake.

*** —— "These crystals of corundum are remarkable for their great irregularity of form and outline, being almost without exception filled with cavities so as to present a mere shell of solid sapphire."

The color of the finest specimens is ruby red, passing into various shades of purple in different specimens : they are translucent to opake. No transparent specimens have yet been found.

The associate minerals are remarkable for their beauty and peculiarities. The following list embraces those which occur in greatest abundance :

Red spinel,	Iron pyrites,
Rose spinel,	Hydrous sesquioxide of iron,
Hydrous silicates of alumina,	Graphite,
Chondrodite,	Phlogopite,
Hornblende,	Emerylite?

The following occur sparingly, viz. rutile, ilmenite, blue fluor.

It is interesting to observe the similarity of the association of the minerals of this locality with those described by Prof. J. Lawrence Smith in his memoirs upon the corundum and emery of Asia Minor, read before the Academy of Sciences of the French Institute, July 15, 1850, and published in the American Journal of Science, vols. x and xii, 2d series.

C. GEOLOGY AND PHYSICAL GEOGRAPHY.

I. PHYSICAL GEOGRAPHY.

1. On the Geographical Distribution of Animals in California. By Dr. John L. Le Conte, of Philadelphia.

During a recent journey through a portion of California, my attention was called to a very remarkable kind of geographical distribution, both of animals and plants, which prevails in that singular portion of our country.

It is well known that the abundance and beauty of California flowers, during the early weeks of spring, is so great, that the whole country appears like a highly cultivated garden : it is difficult for the traveller to believe that the elaborate arrangement of colors, which every where meets his eye, is the result of natural causes; and irresistibly the attention of even the most superficial observer is drawn towards the delicate forms of the vegetation which surrounds him. As compared with the eastern part of the continent, the greater dryness and clearness of the atmosphere have a tendency to repress the foliage of plants, while they develop in a wonderful degree the highly colored appendages which surround the generative organs.

While the attention of the traveller is thus directed to the brilliant color and peculiar form of the flowers, he will not be less conscious of a remarkable phenomenon, which greatly adds to the pleasure of travelling, and, at least in the flower-season, makes full amends for the absence of trees. The flower of to-day will not be seen to-morrow; and even with the slow manner of travelling usually practised in California, a period of two or three days is sufficient to change almost all the plants. Nor is this a change produced merely by difference of latitude or elevation. The plants of the sierra in lat. 32°, with few exceptions, are not those of less elevated regions farther north, and still less are they those of the Sierra Nevada valleys.

This being the case with the vegetation, it became important to determine if similar principles regulated the distribution of animals.

With a view to ascertain this fact, particular note was taken of the localities of all reptiles and insects collected during my journey. These collections were made principally at San Francisco, San Jose, San Diego, Vallecitas (on the eastern slope of the sierra), Junction of the Colorado and Gila, and in the valley of the latter river. The data from the last mentioned region are very imperfect, the collection having been made under peculiarly unfavorable circumstances, and during the coldest season of the year. The mammalia and birds were less noticed, both from the fact that they had been diligently collected by previous travellers, and from a want of sufficient knowledge respecting the objects themselves. For in California, more than in any other country, is the naturalist obliged to rely on immediate observation; the larger objects, especially, in the present condition of the country, must be neglected, as there is no possible means by which they can be preserved, or even taken care of and transported after preservation. I venture, however, from our present knowledge of the higher animals of Western America, to predict, that with the exception of two sets of species hereafter to be mentioned, the results will be found to agree with those derived from the study of the inferior orders.

I cannot avoid, on the present occasion, expressing my sense of deep obligation to the junior officers of the army, stationed at the Colorado and at San Diego. With scarce a single exception, the officers whom I had the pleasure of meeting in California exhibited the most lively desire to aid my researches; not only by the frank hospitality, which has connected with the most frightful deserts some of the happiest reminiscences of my life, but by the free extension of all the facilities which their position could command. Thus was I enabled to visit distant and rarely accessible regions, and to pursue even minute investigations on land never before visited by a zoologist. Though the labor was mine, to them belongs the honor; for without the assistance afforded by these few individuals, who still retain, under the most materializing influences, their appreciation of intellectual pursuits, I feel convinced that a more than ordinary share of scientific zeal could have accomplished absolutely nothing. For this personal favor, as well as for the assistance which they will always take pleasure in extending to scientific men, I beg to offer my most heartfelt thanks.

The first fact observed by the collector, is the very small number of species which can be obtained at any single locality. Day after

day he meets with a continual repetition of a few common forms, with an occasional admixture of rare species; so that at the end of two or three months a single locality will have furnished him with about 200 species of Coleoptera, and a rather less number of other orders. It will be here remembered that the contrary is true of the eastern part of the continent, where each locality furnishes a large number of species, extending over a large area, and represented by comparatively few individuals.

On removing to another locality, the same thing is again observed, with this difference : the species of the first place, even the most abundant, are replaced by others, many of which are true representative species, approaching as closely as those of Eastern America and Europe; while others belong peculiarly to their own district, and are without any representatives in the other parts of the country.

The arrangement of my collection is not yet sufficiently complete, to enable me to determine the numerical proportion of species common to two or more localities; but I may safely assert, that even in adjoining districts it does not exceed 7 or 8 per cent. I may likewise say that those found over a moderate extent of country usually extend over the whole; and the majority of them are either found on this side of the Rocky Mountains, or are represented by species so nearly allied, that it may be doubted if they should not rather be regarded as climatic varieties. It must be observed that the localities east of the Sierra (Vallecitas, Colorado and Gila) show more resemblance in their productions than the maritime regions of California : the desert nature of the country undoubtedly produces this effect, by presenting conditions unfavorable to animal life; yet even in this uniformly sterile tract, great differences are observed among the smaller species which abound only in moist places.

From the distribution of species, I was next led to examine into the nature of the distribution of genera and groups; expecting that a similar geographical position would produce a certain resemblance to the fauna of Europe, and thus vindicate the effects of physical causes in the organic creation.

According to a catalogue made at the time the species occurred, in the principal groups of Coleoptera, there are Carabica 170, Aquatics 70, Silphales 6, Histeridæ 30, Scarabæi 25, Malachidæ 26, Elateridæ 25, Curculionidæ 70, Longicornia 13, Chrysomelidæ 50, Tenebrionidæ 130, Staphylini 135. The whole number of species collected amounted to a little over 1000.

The first point worthy of notice in this list is the extremely small number of Scarabæi, Elateridæ and Longicornia : this might have been predicted, as these insects derive their food for the most part from large plants. The Curculionidæ and Chrysomelidæ are not in the same proportion as in more wooded countries. The saprophagous Coleoptera, with the exception of Histeridæ, are almost wanting; and these latter are not in larger proportion than with us. Thus the only effect, so far observed, is the paucity of species in tribes for which the country affords but little food. The Staphylini and Carabica bear the same proportion to the whole, that they do with us; while the deficiency caused by the small representation of the tribes mentioned above, is made up almost entirely by the Tenebrionidæ, which, as is well known, are but slightly developed in Eastern America. The Malachidæ are also in larger proportion than in other parts of the continent.

Neglecting, on the present occasion, the smaller tribes, let us examine into the constitution of the groups which form the largest portion of the insect fauna of California.

Among the Malachii is one new genus : the others are either Malachius, Collops or Anthicomus, genera distributed through the other parts of the continent.

In the Carabica, but few new genera occur : almost all the species belong to cosmopolitan genera, Cicindela, Lebia, Brachinus, Platynus, Pterostichus, etc. There is then a small number of tropical genera, peculiar to America, and extending a short distance into the temperate zone : Ega, Diaphorus, Lachnophorus. The remainder consists altogether of genera belonging to the temperate zone, and found both in Eastern America and in Europe. Of genera found in the Atlantic States, not in Europe, and likewise of those found in Europe and not in Eastern America, there is not a single species in California*.

The Staphylini contain two new genera : the others are either cosmopolitan, or, if (as Palaminus and Lispinus) American, are also found in the tropics.

The Tenebrionidæ, from being the group most characteristic of the country, might be supposed capable of giving us the most certain

* Chaudoir has described one species of Pasimachus from California; but this genus extends into tropical Mexico.

data with regard to the law of distribution. The great majority of the genera of this tribe are apterous; and of those which are not apterous, all the genera found in California are cosmopolitan (Phaleria, Platydema, Helops, Uloma, Tenebrio, Upis, etc.), except Blapstinus, which again occurs in tropical America. Of the apterous genera, only three are found in eastern temperate America : two of these are peculiar, and one (Nosoderma) which exists in California is also found in Brazil. Of this group, there are in California about 28 genera, of which 5 or 6 extend into the tropics.

The Histeridæ, though not in undue proportion, exhibit a peculiarity : they nearly all belong to the genus Saprinus, which, in Eastern America and Europe, forms scarcely one-fourth of the group.

Thus the only manner in which the insect fauna of California approaches that of Europe, is in the great abundance of apterous Tenebrionidæ. But in this respect it does not differ from a large part of South America; and by the very form of these Tenebrionidæ, which bear no resemblance at all to those of Europe, the greater relation of the Californian fauna to that of the rest of America is clearly proved. It will be seen, too, that the resemblance to European forms in the other tribes is only indirect, proceeding solely from universal or zonal forms, while the greater relation is again with the rest of America. It will moreover be seen, that while the stronger relation of the fauna is continental, yet a sufficient number of individual peculiarities are introduced to prove that it constitutes a system of its own, bearing no relation to that of Eastern America*, except the slight continental resemblance proceeding indirectly through the tropics.

Thus the comparison between the fauna of California and that of the corresponding portion of the other continent, so far from giving any weight to the hypothesis that similar physical conditions will be attended with similar organized products, entirely demolishes this

* I will here incidentally remark, that the insect fauna of the elevated plains of Missouri territory apparently resembles that of California more than that of the Atlantic. This is produced by its resemblance to the fauna of the elevated parts of Mexico, as there are scarcely any species common to the two regions, except such as extend farther east. The Missouri territory is not entitled to rank as a distinct zoological district, being only a prolongation of Central Mexico, with gradual alteration dependent on latitude, and a slight admixture from the neighboring provinces.

view, and shows that the very diverse fauna of different portions of the same continent are yet connected together by a strong similarity of plan.

The peculiarly local distribution of species in California may again be made use of, to illustrate this position. It will be remembered that the researches of Charles Darwin have shown that the productions of the different Gallipagos islands, though having a general resemblance to each other, are yet peculiar, each set to its own island; and that this parallelism between the islands extends through all the classes of organic beings Prof. Gray has, in a former meeting of this learned body, commenced to show that the same is true (at least of the plants) in the Sandwich islands; and we now find a similar local distribution existing on the adjoining continent.

Now, on the ocean, it might be said that the islands are separated by impassable barriers of water, and that there was no opportunity for the species to migrate from one to the other; yet the same kind of distribution has not been observed in the groups of islands connected with the other continent. And now as if to prove a fundamental spiritual relation between the system of distribution and the part of the world in which it occurs, and its entire independence of physical causes, we find in California local zoological districts, no larger than many islands, and separated almost as sharply as if limited by impassable barriers of ocean or ice.

Thus do we see plainly the inadequacy of physical causes to account for even the distribution, much less the origin of organic beings; and we are again led irresistibly to view Nature, in all her manifestations, as the direct expression of an original and harmonious plan of God.

The principles shown by the preceding analysis may be expressed briefly as follows :

1. California constitutes a peculiar zoological district, with sufficient relation to the other districts of America to prove that it belongs to the same continental system.

2. This zoological district is divided into several sharply defined sub-districts, having a very close resemblance to each other.

As the same mode of distribution obtains in the groups of islands adjacent to the western coast of America, we are led to believe,

3. That the local distribution of a small number of species is the characteristic of the eastern Pacific region, as the extensive distribution of a large number is the prevailing feature of the Atlantic.

4. The genera occurring in, but not peculiar to, this district, belong to two classes : either they occur on the Atlantic slope of both continents, or they are peculiar to America, and are also found within the tropics.

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2. PECULIARITIES OF THE CLIMATE, FLORA, AND FAUNA OF THE SOUTH SHORE OF LAKE ERIE, IN THE VICINITY OF CLEVELAND, OHIO. By Dr. J. P. KIRTLAND, of Cleveland.

[*Not received.*]

II. GEOLOGY.

3. REMARKS UPON THE UNCONFORMABILITY OF THE PALÆOZOIC FORMATIONS OF THE UNITED STATES. By Prof. L. AGASSIZ.

PROF. AGASSIZ demonstrated that the palæozoic formations follow each other in regular order on Lake Superior, and in the northern part of America. He alluded to the undulations of the surface of the earth, bulgings and sinkings over extensive areas, producing such phenomena, too much neglected by geologists, as table-lands and low plains. He held to the theory of undulations, instead of that of the upheaval of mountain-chains.

Mr. FOSTER stated that he hardly believed that this subject had attracted the attention which it deserved. Take, for instance, some of the great Coal Measures, and it would be found that there were distinct marks of ten separate deposits, evidently showing that there had been successive upheavals and depressions; for each deposit had its own ripplemarks, which could be traced on what was once the surface. This certainly led to the opinion that there would be a want of conformability where these undulations occurred. Again,

the Galena limestone, which may be found west of the Mississippi, extending to an immense thickness, thins out as you come eastward, till in the State of New-York it is not to be found. It is evidently to be inferred, from the most recent observations, that, while the sedimentary series of rocks were being deposited, undulations may have taken place over areas of vast extent.

Prof. Hall remarked that the Lower Silurian limestones of New-York, which possess a thickness in this State of some six or eight hundred feet, may be recognized along the northern shores of Lakes Huron and Michigan, reduced to some 100 feet; and that still further west, the same rock at St. Anthony's Falls is reduced to a thickness of less than 50 feet. That it is the same rock, we know by the fossils, which are the same species as those found in New-York, and by the strata which occur both above and below. This certainly proves the unconformability of the succeeding strata. Another instance may be cited in the New-York rocks, known as the Utica slate and Hudson-river group, which, in New-York, has a thickness of 800 feet, on Lake Huron but 200, while between Green Bay and the Wisconsin river it totally disappears; and with its disappearance, the fossils, which characterize this rock, are lost also. Another instance of this unconformability is the disappearance of the Medina sandstone and the Oneida conglomerate.

This is one of the difficulties under which Western geologists have to labor; and which will remain, until the various surveys and examinations have identified the rocks of the various western localities, and the fossils which they contain, with those of eastern localities.

The Niagara group can be traced along the north shores of Lake Huron, beyond the Mississippi, into Iowa; and while at Niagara it possesses a thickness of one hundred and fifty or two hundred feet, in the more eastern portions of the State it thins out to four or five feet, while at the west it increases in thickness to a much greater extent; and this, also, forms a proof of the unconformability of the strata.

The Onondaga-salt group, and the series of limestones above, and which in this State have together a thickness of 1800 feet, are entirely wanting west of Lake Huron, while there are some of these formations which may be traced in a southwestern direction as far as Tennessee. The ripplemarks are satisfactory evidence that these depositions have been made in shallow seas during long periods,

during which undulatory movements have operated to raise and depress the surface.

Prof. Agassiz, who, when in Switzerland some years ago, had examined the series of the formations which compose the Jura, found that from the lias to the chalk there were seven different formations; in these the strata were unconformable. He had then been accused of making differences which did not really exist, but time had at length proved that he was correct in his observations. In mountain chains, the strata were always found unconformable.

Dr. Hough stated that in the northern part of New-York the proof of what has been stated is very finely exemplified. Limestone may be seen to thin out to a few feet, between the gneiss and the Chazy limestone.

Prof. Emmons did not believe that the thinning out of certain strata, or a variation in the thickness of a rock, was any proof of unconformability. It was true, the various western strata may not be conformable to the rocks of this State, but it is no proof that the various strata of rocks will not be found to possess conformability when further surveys are made.

Prof. Agassiz said that the total disappearance of a series of rocks, with all their palæozoic remains, ought to be considered as a strong proof of unconformability.

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4. Comparison of the Geological Features of Tennessee with those of the State of New-York. By Prof. James Hall, State Geologist of New-York.

Prof. Hall commenced by stating that our previous knowledge of this region had been derived mainly from the published reports of Prof. Troost, and from a map of Dr. D. D. Owen. From the fossils published by Dr. Troost, it would appear that the lower and upper silurian, devonian, and even carboniferous species occur together in this basin. The lists of fossils given, and which had been cited in foreign publications, showed that there had been no attempt made to distinguish the successive groups as they were elsewhere recognized, and particularly in the State of New-York. The only subdivisions of the lower strata attempted in the West had been into

Blue limestone and Cliff limestone, the rocks above these being all of carboniferous age.

Prof. HALL said, that for several years past, Prof. J. M. SAFFORD had been making examinations in this part of the country, the general result of which he had presented in a geological map, showing not only the limits of this silurian basin, but also the subdivisions which he proposed to make, and which were characterized by certain fossils. The fossils collected by Prof. Safford amounted to some 200 species, about one half of which were identical with those known in the rocks of New-York. In general terms, we might say that the rocks of nearly the whole of this basin corresponded to the lower silurian limestones of New-York. In the lower portion of the series in this basin, Prof. Safford recognizes three divisions. From the lower of these divisions he has brought eight species of fossils; and of these eight species, five are of species characteristic of the Birdseye and Black-river limestones in New-York, and the other three appeared to be new or undescribed species. From the second division, fifty-eight species had been collected, of which twenty-eight were identical with species known in New-York, and mainly those characteristic of the Trenton limestone, a few species only being those which occur in the Birdseye and Black-river limestones. In the upper of these subdivisions, sixteen species had been found, of which eleven were known as characteristic of New-York strata, and nearly all of the Trenton limestone. Among the fossils here enumerated were the *Ormoceras*, the *Gonioceras*, and the *Endoceras*; the *Orthoceras fusiformis*, and other remarkable and characteristic species of lower silurian age, and particularly of the Black-river and Trenton limestones.

Prof. HALL said that he had collected the *Gonioceras anceps* (a cephalopod of peculiar structure) in the Black-river limestone of New-York, and had traced its occurrence in the same geological position as far as the Mississippi river, and it occurred also in the basin of Tennessee. In all the localities it was associated with *Ormoceras* or *Actinoceras*, *Columnaria*, and other fossils which were associated in the same geological position in New-York. It seemed, therefore, scarcely possible to avoid the conclusion that this lower limestone of Tennessee was the equivalent of the Black-river limestone of New-York, and that perhaps also a portion of the Birdseye limestone was included with it. In the second and third divisions there were about forty species of fossils common to those rocks, and

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to the Trenton limestone of New-York; making the conclusion unavoidable, that the strata of these two portions of country were of the same age.

In the same manner the upper or Nashville group of limestones, which Prof. SAFFORD proposed to subdivide into three subordinate groups, contained so many fossils which were identical with those of the Hudson-river group of New-York, or with those which were identical with species common to the Trenton limestone and Hudson-river group, that the conclusion was unavoidable that the two were of the same age. It would seem, however, that some of the Trenton species are found in a lower position than in New-York; and that the subdivision into Birdseye, Black-river, and Trenton limestone, is not as clearly marked in Tennessee as it is further to the east and northeast.

In this connection, Prof. HALL exhibited sections and diagrams, and, by illustrations from a geological map of the United States, showed that the lower silurian limestones, or those known as Chazy, Birdseye, Black-river, and Trenton limestones, thinned out in a northwesterly direction, till they were less than one hundred feet thick on the north shore of Lake Michigan and the west shore of Green Bay; that in Wisconsin, they were less than seventy-five feet thick; and at the Falls of St. Anthony, on the Mississippi river, they were less than fifty feet thick. In Tennessee, the same strata, as far as seen, were two hundred feet thick, and the base had not yet been reached.

In like manner, the Hudson-river group gradually thins out in the west and northwest, its sandy portions disappearing in Canada West; and though eight hundred feet thick in New-York, is less than two hundred on the north shore of Lake Michigan, and disappears entirely in Wisconsin. In Tennessee, the rocks corresponding in position are nearly all of limestone, and have a thickness of about three hundred feet. The strata succeeding these lower silurian limestones are known as the Gray limestone. From this limestone, forty-two species of fossils had been collected, of which twenty-seven were known species, and common to the rocks of New-York as well as Tennessee. But what was very remarkable, was the fact that, of these twenty-seven species, several were of the Niagara group, or those known only in the rocks of that period, while others were known only in the Lower Helderberg limestones; and others still were found only in the Upper Helderberg limestones, or the Onondaga

and Corniferous limestones : thus showing that rocks of the Middle and Upper Silurian periods, and of the Devonian period, were here united in one, or that the formation was altogether so uniform and homogeneous that no subdivision could be made ; that the Onondaga-salt group of New-York, having a thickness of eight hundred or a thousand feet, has entirely disappeared ; and that the Niagara and Lower Helderberg limestones thus come in contact. And again, from the absence of the Oriskany sandstone and the Cauda-galli grit, the Lower and Upper Helderberg limestones are united ; and thus the three limestones, so widely separated in New-York, become physically one limestone in Tennessee.

Prof. HALL showed that each of these periods in New-York were marked by the presence of more than three hundred species of fossils, and that very few of these passed from one group to the next ; showing conclusively that they were distinct formations, and of distinct and succeeding creations. He remarked that this collection of fossils afforded some evidence of the influence of latitude upon the development of animal life, and that climatic influences had prevailed at that early period as well as in subsequent ones. Of the lower silurian species, one half were new, or unknown in the rocks of New-York of the same age, from which four hundred species were already known. In comparing the proportion of new species from the northern and northwestern localities, there were scarcely more than ten per cent of new species ; while at the same distance to the southwest, the proportion was ten times as great. Whether or not this proportion would hold true on further examination, it could not now be determined ; but since so many specimens had been collected, and over such wide areas, it was evident that there were most incontestible proofs of the occurrence of a larger number of species in localities of the same extent at the south, than in northern localities. This difference was therefore to be accounted for either from climatic agencies, or from other circumstances more favorable to the development of species in the southern than in the northern localities.

5. ON SOME OF THE THERMAL WATERS OF ASIA MINOR; WITH AN ACCOUNT OF THE NATURE OF THEIR LOCALITY AND COMPOSITION. By Prof. J. LAWRENCE SMITH, of the University of Louisiana.

THIS is a continuation of a series of examinations on this subject, part of which has already been published. This paper comprises, first, the water of *Yalova*, near the gulf of Nicomedia, and that have been for many ages in more or less repute, and have recently been again brought to notice. They are celebrated in history for the cure of St. Helena, the mother of Constantine the Great. Temperature of water 151° to 156° Fahr. Sp. grav. 1,00115.

COMPOSITION IN GRAMMES.

1 *litre* :	Sulphate of soda	,807
	Sulphate of lime	,414
	Chloride of sodium	,072
	Chloride of calcium	,068
	Sulphate of magnesia	,005
	Sulphate of alumina	trace.
	Silica	,035

A gas escapes from the sources, of which there are about nine. The gas contains, in 100 parts, nitrogen 97, oxygen 3. These waters resemble those of Bath, in England.

Waters of Hierapolis. These are about one hundred and twenty miles south-southeast of Smyrna, and are amidst the ruins of the ancient city of Hierapolis. This place is celebrated for its wonderful encrustations from these springs, which cover the sides of the hill for upwards of a mile in length. The temperature of the water is 130° Fahr., and sp. grav. 1,00143. In one litre, it contains, in grammes,

Bicarbonate of soda	,078
Bicarbonate of lime	1,368
Bicarbonate of magnesia	,041
Chloride of calcium	,020
Sulphate of soda	,341
Sulphate of lime	,119
Sulphate of magnesia	,431
Phosphate of lime	,012
Silica	,008

The encrustation of the spring, in one hundred parts, contains,

Carbonate of lime	,982
Silica	,006
Magnesia Phosphate of lime Fluoride of calcium	,012

Waters of Eski-Shehr. These waters are in the ancient plains of Dorylæum, and about one hundred and thirty miles from the sea of Marmora, and as many south of the Black sea. Its temperature, 119° Fahr.; sp. grav. 1,00017. Contains but a very small amount of solid matter. In one litre,

Bicarbonate of soda	,219
Bicarbonate of lime	,078
Sulphate of soda	,021
Sulphate of lime	,029
Chloride of calcium	trace.
Silica	,008

Thermal Waters of Troy. Near the plains on which this ancient city once stood, are several warm springs, most of them saline. Two were examined, and exhibited the same composition. In one litre, there are, in grammes,

Carbonate of lime	,1225
Sulphate of soda	,0607
Sulphate of lime	,0540
Chloride of calcium	2,5078
Chloride of magnesium	,7031
Bromide of magnesium	trace.
Chloride of iron	trace.
Silica	,0600
Chloride of sodium	17,4450

Thermal Waters of Metylene. On this island there are a number of thermal sources, and the island exhibits abundant evidence of volcanic action. Two sources were examined : the *Kelemyeh Oulinjah* source contains, in one litre, in grammes,

Bicarbonate of lime	,2450
Sulphate of soda	,0857
Sulphate of lime	,0830
Chloride of sodium	,6510
Chloride of calcium	,0865
Chloride of magnesium	,1628
Silica	,0150

The other source examined is called *Souzla :* its temperature is 117° Fahr.; and the composition of one litre is, in grammes,

Carbonate of lime	,0912
Sulphate of soda	1,4625
Sulphate of lime	1,3000
Sulphate of alumina	0,0221
Chloride of sodium	28,0260
Chloride of calcium	2,0040
Chloride of magnesium	0,2023
Carbonate of iron	0,0038
Bromide of magnesium	minute quantity.

6. On the Existence of Diluvial Agencies during the Earlier Geological Periods. By Franklin B. Hough, M. D.

The abrading and polishing action of moving masses of rock, upon superficial strata, has operated so generally, that it may be said to present one of the most common and familiar of geological phenomena, and has usually been considered as peculiar to the drift period and other still later epochs in our earth's history.

The object of this communication is to state a few facts, tending to prove that the causes which produced these appearances have operated in exceedingly remote periods, and at great intervals.

An instance is mentioned by Professor Emmons, in his Agricultural Report, vol. 1, p. 217, in which polished and grooved surfaces occurred between strata of Trenton limestone, at Plattsburgh, and Cumberland Head on Lake Champlain, and at a locality thirty miles south of these places. In these cases the rock above is stated to be in lithological characters and fossils similar to that below, and that the inferior surface of the upper layer presented an exact cast in relief of the one below.

I have observed several instances of a similar character in Northern New-York, which, from the evidence they furnish of the action of denuding causes in early periods, are worthy of special record. One of the most interesting of these localities, and one that would equally repay the visit of the geologist and the tourist, is at Deer River Falls, one mile below the village of Copenhagen, in Lewis county, N. Y.

The river is here precipitated down a chasm in the Trenton limestone, to a depth, it is said, of 270 feet. Near the bottom of these cliffs, the sections in the strata present in several places, and at *different levels*, a peculiar waved line, produced by the removal of portions of the upper surface of a stratum, the lower side of which remains level. The uniformity of stratification is not disturbed either above or below. In several instances the rock above has been removed, presenting continuous furrows, three or four inches deep and a foot broad, with an uniform and parallel course in this instance varying but little from west to east.

The depth and uniformity of these grooves, with the want of polish upon their surface, would indicate that the rock had not acquired solidity at the time of their formation. Quite a number of waved

lines may be seen on the face of the precipice at this place, with intervening level strata many feet in thickness; thus indicating a repetition of the causes, with intervening periods of repose.

A similar occurrence may be noticed in the road one mile west of Champion village, Jefferson county, in which the direction of the furrows is also east and west. Other localities precisely similar may be seen below the bridge on Deer river, between Denmark and Champion; and on both branches of Mill creek, above the village of Lowville. At the last mentioned place, the direction of the furrows is nearly north and south.

The occurrence of these furrows at localities many miles apart, is sufficient to convince the careful observer that they were produced by agencies that operated with uniformity over extensive surfaces.

All of these abrasions occur near the base of the Trenton limestone; and at Lowville, they are but a few feet above the stratum corresponding with the Isle la Motte marble.

A more interesting and characteristic locality of furrowed surfaces underlaying rocky strata, was first noticed by Mr. W. E. Guest, of the village of Ogdensburgh, about a mile above that place, near the St. Lawrence river, and between strata of Calciferous sandstone. The rock below this is a coarse gritty sandstone, mostly without fossils; while that above is an impure limestone, containing the *Euomphalus uniangulatus*, a fossil characteristic of the Calciferous sandstone, and several undetermined coralloids. It forms a durable building stone, and has been long used in making lime. The specimens exhibited were procured at this place, and present, as may be seen, two series of scratches crossing at an angle of about 45°. The deeper and older grooves extend from NNE to SSW, while the slighter and more recent ones extend from 10° W of N to 10° E of S. The marks of attrition are quite as distinct as is ever noticed on superficial rock, and were evidently made after the stratum had acquired a solidity quite equal to that which it now possesses. At one point on the shore of the St. Lawrence, the point of contact between the strata which contain the scratches may be seen at low water.

At the locality where the specimens were procured, the furrows are seen passing horizontally under an abrupt terrace of the coarse limestone about twenty feet high; but the exact point of contact was not seen, it being covered by a wall. The surface of the rock

above exhibits here no marks of abrasion, although elsewhere it preserves them with great distinctness. After a careful study of the locality, I have not the slightest doubt but that the terrace of limestone overlays the grooved and polished rock in question. At a locality one mile east of the village of Hammond, in St. Lawrence county, an appearance quite similar may be noticed.

These instances, although insufficient to decide the disputed question of the manner in which the abrading forces acted, at least indicate that they existed, and operated with the same effect, and doubtless under the same circumstances, during the eras of the Calciferous sandstone and Trenton limestone, as in the Drift period.

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7. On the Terraces and Sea-Beaches that have been formed since the Drift Period, especially those along the Connecticut River. By Edward Hitchcock, D.D., LL.D., President of Amherst College.

Two years ago, I called the attention of the Association to this subject, soon after I commenced its examination. Since that time I have been collecting facts, both in this country and in Europe, and attempting to mature my conclusions. Last year I presented the subject briefly to the British Scientific Association, at their meeting held in Edinburgh. I do not propose in this paper to attempt, to much extent, a detail of the facts collected, but rather to state the leading conclusions to which I have been brought, with a brief summary of facts. Terraces are of three kinds:

1. *River Terraces*, the most perfect of all; formed along the sides and at the mouths of rivers, and especially in basins. Of four kinds: 1. The Lateral terrace; 2. The Delta terrace; 3. The Gorge terrace; 4. The Glacis terrace, the latter the most perfect in the vallies of the Alps. Most of the details in this paper relate to River terraces.

2. *Lake Terraces*. Sometimes, as in the small lakes of New-York, these seem to be merely the lateral terraces of an expansion of a former river; sometimes, more probably, ancient beaches, on shores of the lake, as around our larger lakes.

3. All terraces that cannot be referred to rivers or lakes, must be

considered as Sea beaches. The most perfect are those now existing along our shores. As we ascend they become less distinct, until the highest cannot be distinguished from drift.

General Characteristics of the Terraces and Sea Beaches.

1. The most perfect terrace is an alluvial meadow, which I call the lowest terrace.

2. Ascending from the meadow, we find successively terraces of clay and sand, of fine pebbles, of coarse pebbles; then a sea beach of sand or gravel, with boulders intermixed; and, finally, a coarse drift, highest and deepest of all. (Refer to drawings.)

Origin of the Materials.

1. Mainly from the drift; so that terraces may usually be regarded as modified drift.

2. From erosions of the rocks by water and ice.

Details of Facts.

1. *In this country.* My researches have been mostly confined to the Northern States, and mainly to New-England.

[Show a map of the terraces along Connecticut river; also of the basin of Deerfield river, and of Westfield river; around the gorge at Brattleboro', and at Bellows falls; along Fort river in Amherst and Pelham.]

2. *In Europe.* In Wales, few terraces, but decisive marks of ancient glaciers.

In England, terraces not well marked. Beaches not unfrequent.

In Scotland, the terraces and associated phenomena resemble those in the United States.

In Germany, along the Rhine, are terraces; ex. gr. between Bonn and Cologne (near the former), a terrace 15 or 20 feet high, formed by the river gradually changing its bed. Above these are beds of coarse sand and gravel. Between Bonn and Mayence are numerous examples of terraces, rarely more than two; sometimes beaches above them. Terraces not unfrequent in the wider parts of the valley above Bingen. Beyond Heidelberg to Basle, terraces are occasionally seen, and now and then a beach at a higher level. Above Basle, terraces are numerous, the two lowest very distinct: beaches

higher up. Around Bruges, where the rivers Reuse and Limmat unite with the Aar, terraces are fine. The highest terraces on the Rhine, above Basle, are as much as 300 feet above the river, and 1200 feet above the ocean. I noticed beaches certainly as high as 940 feet above the river, and 1915 above the ocean. Three or four terraces around Lake Zurich : the highest I found 380 feet above the lake, and 1734 above the ocean. (Zurich lake 1342 feet above the sea.) An imperfect beach 2165 feet above the ocean.

Between Lucerne and Berne, terraces are abundant; the highest 1705 feet above the sea, and 325 above the lake, with beaches above them : say the highest 2274 feet above the sea, Lucerne being 1380 feet.

Between Berne and Vevay, terraces are frequent, with sea beaches away from the rivers. Highest beach (Berne 1856 feet) 2667 feet above the ocean.

Three or four terraces (not fully continuous) on the north and west shores of Lake Leman, begining at Lausanne and reaching to Geneva.

Between Geneva and Chamouny, terraces are numerous along the Arve. Between Lake Leman and the Arve, the highest is 140 feet above the lake, and 1370 above the ocean. At Sallenche the highest is 580 feet above Leman, and 1810 above the ocean. At Chamouny are some terraces (in a basin) 3270 feet above the ocean. At Argentière are others 4100 feet, and at La Tour others 4353 feet above the ocean. On the Eau Noire, in the pass of Tête Noire, the highest is 4218 feet. None on the Rhone around Martigny : valley remarkable.

Phenomena in Switzerland and Sardinia similar to those in North America, except that modern glaciers have covered the terraces in some places with moraines. In the great valley of Switzerland, I saw but slight traces of former glaciers : the drift there mostly modified as in this country.

In Scandinavia, the surface has been subject to a *seesaw* movement since the terraces were formed (See a paper by Robert Chambers, in the New Edinburgh Philosophical Journal for January, 1850).

In Ireland, *osars* are common, as well as unmodified drift.

CONCLUSIONS.

1. For the most part, the terraces and beaches lie above the unstratified drift, and were therefore subsequently formed; yet in some instances the drift is mixed with the laminated and modified materials, and erratic blocks are strewed over the same, shewing that the drift agency recurred occasionally in connection with the modifying agency.

2. These beaches and terraces have been formed chiefly by the action of water, modified in some cases, perhaps, by ice. PROOF 1. The sorting of the materials, so that the fragments or pebbles should be only of a certain size in the different terraces. 2. The horizontal arrangement and more or less level tops of the terraces, and glacis-form of the beaches. 3. The rounded form of the coarser fragments, and the comminuted state of the finer portions of the terraces. 4. The large boulders, and especially the trains of angular fragments sometimes mixed with, or strewed over the terraces and beaches, as well as those rounded hills and depressions and ridges sometimes called iceberg moraines, indicate the presence of ice as icebergs, in the waters.

3. The beaches, and perhaps some of the highest terraces, were formed by the waves and currents of the ocean; while the lower terraces were formed in lakes, or the expansions of our present rivers as they gradually wore away their barriers. PROOF 1. If the waters rose high enough to form the beaches, they must have been oceanic, because these beaches are so high that no barrier could have been interposed between them and the present ocean. 2. Many of the most recent terraces are so low, and so cut off by rocky barriers from the ocean, save through the gorge of a river, that the waters of the sea could not have formed them; because it could have reached them only through that rocky gorge.

4. The whole work seems to have been the result of the gradual and slow drainage of the country as it rose from the ocean, or the ocean withdrew from it.

5. A slow and equably vertical movement of the continents or the waters is sufficient to account for the formation of most of the beaches and terraces, without supposing the sudden bursting of barriers, or pauses in the movement. PROOF 1. Show how river terraces are now forming by the streams wearing away one of their

banks, and depositing a meadow on the opposite side. 2. Show how delta terraces might have been formed, by the gradual withdrawment of the waters of a lake, or estuary, laying bare a terrace; through which the river must now cut its way. In doing this it would also form lateral terraces, in the way they are now forming in alluvial regions. A case of this sort has actually occurred in Switzerland (See Chambers' pamphlet on the terraces of the Rhine and the Rhone). 3. Lateral terraces might be formed below the mouth of a tributary stream, by the current of the main river carrying downwards some of the materials brought down to form the delta. 4. When the highest terrace had been laid bare at the mouth of a river, the waters would carry forward materials to form a second terrace at a lower level. If the principal stream continued to deepen its bed, this terrace would at length reach the surface, and in the same manner a third and a fourth might be formed. 5. Many lakes, like those in New-York, seem to have been only expansions in rivers, and terraces may have been formed along their borders as above explained. 6. The sediment which is now being carried into the ocean by the rivers, is swept lengthwise of the coast, and forms a level-topped terrace : if the land is rising, it will at length reach the surface when no more sediment can be deposited upon it; but the sediment will be carried into deeper water, where a new terrace will be formed; while the breakers, as the ocean sinks, will act on the margin of the upper terrace and make it steep. In the same way the terraces may have been formed around our larger lakes, as their outlets were gradually deepened.

6. Paroxysmal vertical movements may occasionally have occurred, while these slow operations have gone on; and these would modify the processes, and produce, it may be, some terraces, ex. gr. the Parallel Roads of Glen Roy.

7. It is difficult, if not impossible, to draw a line between drift and modified drift, such as constitutes beaches and terraces. Indeed, the drift agency seems to have resumed its activity in some places, long after the beaches and terraces began to be formed; ex. gr. 1. Where coarse drift and clay or sand are intermixed; 2. The trains of blocks in Richmond, Mass.

8. Hence the drift agency, and that which formed the beaches and terraces, appear to be only modified forms of the same agency. 1, In forming the coarse drift, ice appears to have been the princi-

pal agent. No one, I should think, could examine the Alps, without being satisfied of this. But still in our country the coarsest drift not unfrequently shows the conjoint action of water in sorting and laminating the materials. The aqueous action seems to have increased, and that of ice to have diminished until it has ceased, save in winter floods. 2. In the Alps and Wales we see drift formed wholly by glaciers; but such drift differs a good deal from that of the United States, of Scotland, and even of the great valley of Switzerland. We have no genuine moraines in this country: what have been called such, are modified drift. 3. If we could get access to the floor of the Arctic ocean, I fancy that we should find that coarse drift was forming there, similar to what is strewed over our country; though it may yet be proved that glaciers have existed on some of our mountains as they rose from the sea. Between the enormous icebergs of the Northern ocean and glaciers, there can be little difference—I mean as to their action and effects—save that we should see the action of water, modifying somewhat that of ice, in the case of icebergs.

9. According to these views, we see the reason why the older drift is entirely destitute of organic remains. Those said to occur in it (ex. gr. in Vermont, Brooklyn, Portland, &c.), belong to a very late period of alluvial agency.

10. Hence drift and alluvium constitute only one distinct and uninterrupted formation.

11. Hence the alluvial formation may be of different ages in different countries, depending upon the time when they were raised above the ocean. The deposite may be nearly as old as the rock over which it is spread; which, in our country, embraces the whole geological series.

12. The time that has elapsed since the drift was formed, has been immense. Proof 1. Scarcely the lowest of the terraces has been formed since the memory of man. 2. Cases of erosions show the same; but these will be better described in my paper on "Erosions."

13. The Historic Period can have been only a small portion of the Alluvial Period.

8. On the Geology and General Character of the Meerschaum in the Plains of Eski-Shehr (Asia Minor). By Professor J. Lawrence Smith.

This substance is found in the ancient plain of Dorylæum, now called *Eski-Shehr.* It is found at various depths in a species of calcareous breccia, containing masses of the rocks of the surrounding mountains, where we find all that is found in the plain, except the meerschaum, the origin of which I am inclined to attribute to a change produced upon carbonate of magnesia by waters containing silex. It was doubtless explored at this very place by the ancient Greeks : the use, however, that they made of it, is unknown to us. The companies who now explore it are Turks, and those who labor are paid proportional to what is extracted ; and as the value of this substance increases greatly in proportion to the size of the mass, the business is of a very precarious nature, and in many instances is a cause of great loss to the miners. At other times they procure pieces affording large marketable specimens, and their profits then are proportionally good. Its mining is carried on with the same eagerness, and its yield is as precarious as that of gold.

[Specimens were exhibited, showing both its mineralogical and geological character.]

The principal mines in the plain are *Remicklich,* twenty-seven miles north of Eski-Shehr, where the shafts are from ninety to one hundred and twenty feet deep ; *Káráneh,* fifteen miles east of Eski-Shehr, which is an old Greek mine, and has shafts from sixty to ninety feet deep ; *Nembekerer,* fifteen miles west of Eski-Shehr, and one near *Rutayah.* The two first are worked at present. Káráneh, recently recommenced, is yielding well ; and the third is abandoned, on account of the rapidity with which the water flows into it.

In sinking a shaft in these places, the meerschaum is found from the surface down ; but that near the surface has not the requisite properties, being hard and chalky, and does not yield readily to the pressure of the nail as good meerschaum does.

9. Remarks on the Lithological and Palæontological Characters of the Potsdam Sandstone. By T. S. Hunt, of the Geological Commission of Canada.

The formation designated by the geologists of New-York as the Potsdam sandstone, is displayed along the base of the crystalline rocks from Mal Bay thirty leagues below Quebec, to near Kingston at the outlet of Lake Ontario. The Rideau canal, which extends from Kingston to Bytown, cuts across the western edge of a lower silurian basin which lies between the Ottawa and St. Lawrence rivers. Upon Lake Huron, this formation is met with on the northern shore of the lake, resting aimost horizontally upon that series of unconformable copper-bearing rocks which has been described by Messrs. Logan and Murray as consisting of quartz rock interstratified with slates, limestones, conglomerates and greenstones, and which reposes upon the formation of syenitic gneiss and crystalline limestone which extends from Labrador to Huron, and northwest to the Arctic Sea.

From the outcrop of the sandstone which is seen on the Rideau canal, it is traced around the south side of the basin towards the east, where it is developed on the western shore of Lake Champlain, and is known as the Potsdam sandstone. Throughout this extent it is generally undisturbed; but, as might be expected, differences of lithological character are met with : from the inequalities of surface in the underlying unconformable rock, moreover, a difficulty is often met with in ascertaining the character of those beds which may really be at the base of the formation. Generally, however, it may be asserted that beds of conglomerate lie at the base in its eastern development. As we go westward, the distinction in lithological character between this rock and the overlying calciferous sandrock is less defined, and somewhat calcareous beds are met with in the former; while upon Lake Huron, the formation is represented by only about forty feet of reddish and greenish sandstones and marls, which are considerably calcareous towards the top; and at Sault Ste. Marie there is a large mass of reddish sandstone, in some parts conglomerate in its character.

In the western part of the silurian basin which lies between the Ottawa and the St. Lawrence, the sandstone over limited areas is sometimes strongly impregnated with peroxide of iron, which is occasionally found in thin seams or beds in a pure state, constituting

red hæmatite. These features are explained by a reference to the ferruginous springs which are still found issuing from the crystalline rocks of the country, and which often cement large areas of gravel with the deposited oxide, and sometimes form beds of limonite and iron ochre.

Hitherto no fossils in this formation had rewarded our search, other than those described and figured by Mr. Hall; until a year since, when the attention of Mr. Logan was called by Mr. R. Abraham, of Montreal, to certain footmarks upon the beds of this sandstone, at Beauharnais. Casts of these, and specimens of the stone itself, bearing the footprints, were carried last winter by Mr. Logan to London, and submitted to the critical examination of Prof. Owen, whose expressed opinion that they are those of a reptile, and probably a chelonian, is well known to the members of the Association. I have only to add that this season similar tracks have been discovered in large numbers, and at distances several miles apart. There is a great diversity in their size, but they agree in their general characters*. The beds containing them are lying at a very small angle, and are overlaid by undoubted calciferous standstone, containing two species of *Maclurea*, one the *M. matutina* figured by Mr. Hall; while to this formation succeeds the Trenton and its associate limestones, with their characteristic fossils.

Mr. Hunt remarked that although the discovery of traces of a reptile in the lowest rocks, now recognized to be fossiliferous, was a new and startling fact for those who have supported the idea of progressive development, it was one for which the progress of modern geology had, to some extent, prepared us. He alluded to the remains of fishes which, for about twenty years, were unknown below the coal, but have been successively brought down through the Devonian, Upper and Lower Silurian formations. The remains of fishes are more recently announced as existing even in the Bala limestones†, which may be considered the analogues of some of the lower portions of our silurian rocks, if we regard the Potsdam sandstone as the representative of the Obolus sandstone in Russia, and the Lingula beds of Wales.

* Since the above was written, the examination of a large number of these footmarks has led Prof. Owen to change his opinion, and to declare them to be the traces of crustaceans.

† The fossils of the Bala limestone, upon which this conclusion was based, have since been shown to be fragments of crustaceans, and not fishes.

The rare occurrence of animal remains in these sandstones which, in Russia and Scandinavia, as well as in America, are found at the base of the formations hitherto recognized as fossiliferous, is, perhaps, one reason why they have hitherto been regarded as dating from the very commencement of animal existence; but when we remember that the superior sandstones of the silurian rocks, the old and new red, are almost equally destitute of animal remains, we are inclined to look for some other reason to explain their absence from siliceous deposites. The remains of vegetables are not unfrequently found in them, and the bony skeletons of fishes embalmed in the bituminous product of the decomposition of their integuments are found in a high state of preservation; but the phenomena which we have to explain, is the absence of the calcareous coverings of mollusca and crustaceans. And this, I think, may be accounted for by the action of atmospheric waters charged with carbonic acid, which are capable of taking into solution a considerable quantity of carbonate of lime. The abrading action of a sharp siliceous sand is also to be taken into account, which, where it does not destroy the shells, renders them more easily affected by the solvent. Such being the case, it would only be in those beds which contain a portion of finely divided calcareous matter, presented to the water in a readily soluble form, that we might expect to find the shells escaping the dissolving action; and conformably to this, we find that those beds of the Potsdam sandstone which are somewhat calcareous, contain, in great profusion, their peculiar fossil shells. These considerations, coupled with the abundance of fucoidal remains and the footmarks lately discovered, permit us to suppose that this lower formation was not, after all, so destitute of organic life as has hitherto been imagined. And when I add, that there is not wanting evidence of the sedimentary origin of the crystalline limestones and syenitic rocks which underlie these sandstones, it will be conceded that neither the inferior position of the formation, nor the paucity of organic remains, can be adduced as arguments against the supposition that its epoch was one prolific in living existence, and, perhaps, long subsequent to the commencement of life upon our planet.

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10. ON THE METAMORPHIC COALFIELD OF MASSACHUSETTS. By President E. HITCHCOCK.

[*Not received.*]

11. On the Passage of Anticlinal Axes into Faults. By Prof. W. B. Rogers, of the University of Virginia.

[*Not received.*]

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12. On the Geology of Emery and Corundum. By Prof. J. Lawrence Smith.

This communication is meant simply for the purpose of introducing a series of specimens demonstrating the geology of that important mineral, emery. The mass of observations made by me in Asia Minor have already been made public, so that only such additional observations as have been made are here given. The constant occurrence of corundum, emery, and its associates in the old crystalline limestone, has since been observed in this mineral coming from Siberia, South America, and all parts of the United States; and with it has been found more or less of these minerals, as I had stated would be, in all probability, after the observations made in Asia Minor.

From Liberia, corundum has been obtained, associated with tourmaline and emerylite; and doubtless, could observations be made on the spot, many, if not all the minerals that I call associate minerals of emery, would be found.

In the United States there has been found emerylite, hydrargilite, pholerite, titaniferous iron, and tourmaline associated with corundum.

The process of the formation of corundum, during the consolidation of the limestone on which it is found, has been also observed in this country, strengthening the observations made in Asia Minor. The process of segregation is seen beautifully in the corundum of New-Jersey, where segregated masses of hydrated alumina are found, in the centre of which are seen crystals of corundum. As yet, in none of the corundum localities of this country have I been able to find emery; which is a curious fact, not easily explained, especially as the same thing is true with reference to localities of the corundum of other countries.

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13. Notes on the Geological Structure of Western Vermont and Massachusetts. By Prof. W. B. Rogers, of the University of Virginia.

[*Not received.*]

14. On the Distribution of Manganese. By D. A. Wells, Esq., of Cambridge.

The occurrence of pebbles and water-worn stones in many of the streams and water-courses of New-England, which have their origin among and run over igneous and metamorphic rocks, is by no means uncommon, and has doubtless attracted the attention of every observer. When the bed of a stream in which they occur is examined, the colored pebbles and stones will be found at intervals, generally after, or below a fall or rapid, and not immediately above. This coloring matter, which is wholly superficial, and of different degrees of lustre, is due to an incrustation of the black oxide of manganese, and occurs independently on almost every variety of stone.

In the Edinburgh New Philosophical Journal for July, 1851, Dr. John Davy calls attention to somewhat similar incrustations in England, of which he says as follows : "Though always superficial, in one spot, the incrustation is so thick as to be available for use; and in this instance the black oxide of manganese acts as a cement, forming a bed of conglomerate several feet thick. Whence this incrustation is derived, or how produced, is not obvious. Restricting the view to the spots where it occurs, it might be supposed to be a deposit from running water. But when it is seen that the coloring matter is not to be detected on rocks *in situ*, the fixed rocks in the course of the stream, the idea ceases to be tenable; and the inference seems to be unavoidable, that the sand, pebbles and stones thus colored have been incrusted with the oxide before they had been carried down to the spots where they are found loose; or when in the form of conglomerate, that the cementing oxide has been brought there by water exuding from some rock or stratum containing manganese in a minor degree of oxidation, and acquiring the higher degree by the absorption of oxygen, and at the same time the cementing quality." Dr. Davy also infers from such incrustations, that the oxide of manganese exists in the vicinity in large quantities, and advises a special inquiry in search of it.

Before the publication of the article referred to, by Dr. Davy, the subject of these incrustations had attracted the attention of Dr. A. A. Hayes of Boston, and myself; and we believe the following to be a full and satisfactory account of the origin of this phenomenon :

The manganese exists in almost all the igneous and metamorphic rocks of New-England, and I may say in other parts of the world, generally in form of a double carbonate of lime and manganese. When the waters of the springs, ponds and rivers flowing over these rocks, become charged with soluble organic matter, in the state of crenic, apocrenic or humic acids, drained into them from swamps and peat-meadows in consequence of rains or inundations, the carbonates of lime and manganese enter into solution. At such times manganese may be generally detected in these waters, as has been done by Dr. C. T. Jackson, Dr. Hayes, and others. When the water holding the manganese in solution becomes broken and thrown up in the passage of falls and rapids, and consequently exposed to the influences of the atmosphere, the manganese passes from a low state of oxidation to the insoluble peroxide, and is deposited for a considerable extent upon the rocks and pebbles below. It thus upon examination will be found, that at intervals in the bed of the stream, the stones are completely blackened or discolored, while in other places no such depositions can be observed. Beautiful examples of this phenomenon may be seen at some points upon the Merrimac river, and indeed in almost every brook and rivulet in New-England. I have also noticed similar depositions between the divisional strata planes of sandstones in the valley of the Connecticut; thus showing that apparently the same agencies were at work during the deposition of these rocks, as are at the present day.

As an example of the extent to which manganese occurs in some of the metamorphic rocks of New-England, I submit an analysis of an altered rock, occuring somewhat extensively in the neighborhood of Nahant. The analysis was made at my request by Mr. John Hague of the Cambridge Laboratory, and afterwards verified by Mr. Joseph Ela. Its composition was as follows :

$Si\,O^3$	52,17
$Fe^2\,O^3$	9,78
$Mn^2\,O^3$	26,72
$Al^2\,O^3$	8,43
$Ca\,O$	0,37
$Mg\,O$	0,60
$H\,O$	2,02
Total......	100,09

15. On the Geological Agency of the Winds. By Lieutenant M. F. Maury, U. S. Navy.

Nature is a whole, and all the departments thereof are intimately connected. If we attempt to study in one of them, we find ourselves tracing clues which lead us off insensibly into others, and, before we are aware, we find ourselves exploring the chambers of some other department.

The study of drift takes the geologist out to sea, and reminds him that a knowledge of waves, winds and currents, of navigation and hydrography, are closely and intimately connected with his favorite pursuit.

The astronomer directs his telescope to the most remote star, or to the nearest planet in the sky, and makes an observation upon it. He cannot reduce this observation, nor make any use of it, until he has availed himself of certain principles of optics; until he has consulted the thermometer, guaged the atmosphere, and considered the effect of heat in changing its power of refraction. In order to adjust the pendulum of his clock to the right length, he has to measure the water of the sea and weigh the earth : he too must therefore go into the study of the tides; he must examine the earth's crust, and consider the matter of which it is composed from pole to pole, circumference to centre; and in doing this, he finds himself in his researches right alongside the navigator, the geologist and the meteorologist, with a host of other good fellows, each one holding by the same thread, and following it up to the same labyrinth, where are stores of knowledge for all, and instruction for each one in particular. And thus, in undertaking to follow the "wind in his circuits" over the ocean, I have found myself standing side by side with the geologist on land, and with him far away from the sea-shore, engaged in considering some of the phenomena which the inland basins of the earth—those immense indentations on the surface that have no sea drainage—present for contemplation and study.

Among the most interesting of these, is that of the Dead Sea. Lieutenant Lynch, U. S. Navy, has run a level from that sea to the Mediterranean, and finds the former to be about 1300 feet below the general sea level of the earth. In seeking to account for this great difference of water-level, the geologist examines the neighboring region, and calls to his aid the forces of elevation and depression

which are supposed to have resided in the neighborhood: he then points to them, as the agents which did the work. They are mighty agents, and they have diversified the surface of the earth with the most towering monuments of their power. But is it necessary to suppose that they resided in the vicinity of this region? May they not have been, if not in this case, at least in the case of other inland basins, as far removed as the other hemisphere? This is a question which I do not pretend to answer definitively. But the inquiry as to the geological agency of the winds in such cases, is a question which my investigations have suggested; and I therefore present it as one which, in accounting for the formation of this or that inland basin, is worthy, at least, of consideration.

Is there any evidence that the annual amount of precipitation upon the water-shed of the Dead Sea, at some former period, was greater than the annual amount of evaporation? If yea, where did the vapor that supplied that precipitation come from, and what has cut off that supply? The mere depression of the lake bed would not do it.

If there were ever a river from the Dead Sea to the arms of the ocean about it, we may imagine that river to have abounded with falls, as the rivers do which drain the lakes into the Atlantic. And if we establish the fact that the Dead Sea did ever send a river to the ocean, we carry along with it the admission, that when that sea overflowed into that river, then the water that fell from the clouds over the Dead Sea basin was more than the winds could convert into vapor and carry away again: the river carried off the excess to the ocean.

In the basin of the Dead Sea, in the basin of the Caspian, of the Sea of Aral, and in the other inland basins of Asia, we are entitled to infer that the precipitation and evaporation are at this time exactly equal. Were it not so, the level of these seas would be rising or sinking. If the precipitation were in excess, these seas would be gradually becoming fuller; and if the evaporation were in excess, they would be gradually drying up: but observation does not show, nor history tell us, that either is the case. As far as we know, the level of these seas is as permanent as that of the ocean, and it is difficult to realize the existence of subterranean channels between it and the great ocean. Were there such a channel, the Dead Sea being lower, it would be the recipient of ocean waters; and we

cannot conceive how it should be such a recipient, without ultimately rising to the level of its feeder.

It may perhaps be evident that the question suggested by my researches has no bearing upon the Dead Sea; that local elevations and subsidences alone were concerned in placing the level of its waters where it is. But is it probable that, throughout all the geological periods, during all the changes which have taken place in the distribution of land and water surface over the earth, the winds, which in the general channels of circulation pass over the Dead Sea, have alone been unchanged? Throughout all ages, periods and formations, is it probable that the winds have just brought as much moisture as they now bring, and have just taken up as much water from it as they now carry off? It is possible. But because the agency of the winds may have had nothing to do in placing the level of the Dead Sea where it is, does it therefore follow that the consideration of this question would be irrelevant in the attempt to account for the level of the water reservoirs of other inland basins?

Where does the water, which falls from the clouds upon the valley of the great North American lakes, come from? It goes into the sea, and out of the sea it must come again; else "the sea would be full." From what part of the sea, therefore, do the clouds get vapor to make rain of for the lake country?

The researches conducted at the National Observatory with regard to the winds, have suggested the probability that the vapor which is condensed into rains for the Lake valley, and the excess of which the St. Lawrence carries off to the Atlantic ocean, is evaporated by the SE trade winds of the Pacific ocean. Suppose this to be the case, and that the winds which bring this vapor arrive with it in the lake country at a mean dew point of 50°. This would make the SW winds the rain winds for the lakes generally, as well as for the Mississippi valley: they are also, speaking generally, the rain winds of Europe, and, I have no doubt, of extra-tropical Asia also.

Suppose a certain mountain-range, thousands of miles to the SW of the lakes, but across the path of these winds, were to be suddenly elevated, and its crest pushed up into the regions of snow, having a mean temperature of 30° Fahr. The winds, in passing that range, would be subjected to a dew point of 30°; and not meeting with any more evaporating surface between such range and the lakes, they would have no longer any moisture to deposite at the supposed lake temperature of 50°: they could not yield the dew

point to anything above 30°. Consequently the precipitation in the lake country would fall off; the winds which feed the lakes would cease to bring as much water as the lakes now give to the St. Lawrence; that river, and the Niagara, would drain them to the level of their bed; evaporation would be increased, by reason of the dryness of the atmosphere and the paucity of rain; and the lakes would sink to that level, at which, as in the case of the Caspian Sea, the precipitation and evaporation would become equal. Thus, our great lakes would remain inland seas at a permanent level; the salt brought from the soil, by the washing of the rivers and rains, would cease to be taken off to the ocean as it now is; and finally, the Great Lakes too, in the process of ages, would become first brackish, and then briny. Now suppose the water basins which hold the lakes to be over a thousand fathoms, 6000 feet deep. We know they are not nearly so deep; but suppose them to be 6000 feet deep. The process of evaporation, after the St. Lawrence had gone dry, might go on until one or two thousand feet or more were lost from the surface; and we should then have another instance of the level of an inland water basin being far below the sea level, as in the case of the Dead Sea; or it might become a rainless district, where the lakes themselves would go dry.

Corallines are at work about the Gulf stream: they have built up the Florida reefs on one side, and the Bahama banks on the other. Suppose they should build up across that pass, and obstruct the Gulf stream; and that in like manner they were to connect Cuba with Yucatan, by damming up the Yucatan pass, so that the waters of the Atlantic should cease to flow into the Gulf. What should we have?

The depth of the marine basin which holds the waters of the Gulf, is, in the deepest parts, about a thousand fathoms. The officers of the U. S. Ship Albany have run a line of deep sea-soundings from west to east across the Gulf: the greatest depth they obtained was 960 fathoms (5760 feet).

We should therefore have, by stopping up the channels between the Gulf and the Atlantic, not a sea-level in the Gulf, but we should have a mean level between evaporation and precipitation. If the former were in excess, the level of the Gulf waters would sink down until the surface exposed to the air would be just sufficient to return to the atmosphere, as vapor, the amount of water discharged by the rivers, the Mississippi and others, into the Gulf. As

the waters were lowered, the extent of evaporating surface would grow less and less, until nature should establish the proper ratio between the ability of the air to take up, and the capacity of the rain to let down. Thus we might have a sea whose level would be much further below the water level of the ocean, than is the Dead Sea.

There is still another process besides the two already alluded to, by which the drainage of these inland basins may, through the agency of the winds, have been cut off from the great salt seas; and that is by the elevation of continents from the bottom of the sea in distant regions of the earth, and consequently the substitution of a dry land for a water surface as the sources of vapor supply to the winds that blow over the place.

From what part of the ocean, I again ask, comes the vapor which forms the rains that fall on that immense water-shed to which the Lakes give drainage? My investigations have suggested the idea that they come from the trade-wind region of the South Pacific ocean. Certain it is, that they must come from the sea, and not from the land; for in this view, I do not consider that the rain which falls to-day is taken up straightway into the clouds to be precipitated to-morrow; but I consider the excess of the precipitation over the evaporation, which in this case is the volume of water discharged by the St. Lawrence into the sea; that is, the amount of water which has to be taken up from the sea again, carried back through the air to the Lake country, and precipitated upon it. And I therefore repeat the question : Where, from what portion of the ocean is it taken up into the air? It must be taken up from some portion where the evaporation is greater than the precipitation; and that is only in the trade-wind region : and it must also be taken up where the mean temperature, or at any rate where the mean dew point is higher than it is in the Lake country; for after moisture gets into the atmosphere, it is only by lowering the dew point, that we can get it out again.

Now suppose that a continent should rise up in that part of the ocean, wherever it may be, that supplies the clouds with the vapor that makes the rain for the Lake water-shed : What would be the result? Why surely a change of climate in the Lake country : an increase of evaporation; because a decrease of precipitation, and consequently a diminution of cloudy screens to protect the waters of the lakes from being sucked up by the rays of the sun; and conse-

quently, too, there would follow a low stage for water-courses, and a lowering of the lake level.

So far, I have used the lakes only hypothetically, that I might the better illustrate the bearings of the question with which I set out, viz : Where have the subsidences and the elevations taken place, that have made an inland basin here, and another there? Is the seat of this action near by, or far off; and what have the winds had to do in cutting off the sea drainage of inland water-sheds?

But in this hypothetical case, with regard to the basins of the Gulf and Lakes, I have confined myself strictly to analogies. Mountain ranges have been upheaved across the course of the winds, and continents have been raised from the bottom of the sea; and, no doubt, the influence of such upheaving has been felt in remote regions by means of the winds, and the effects which a greater or less amount of moisture brought by them would produce.

In the case of Utah, we have an example of drainage that has been cut off, and an illustration of the process by which nature equalizes the evaporation and precipitation. To do this, in this instance, she is salting up the basin which received the drainage of this inland water-shed. Here we have the appearance, I am told, of an old channel by which the water used to flow from this basin to the sea. Supposing there was such a time and such a water course, the water returned through it to the ocean was the amount by which the precipitation used to exceed the evaporation over the whole extent of country drained through this, now dry, bed of a river. The winds have had something probably to do with this : they are the agents which used to bring more moisture to this water-shed, than they took away; and they are the agents which now carry off from that valley, more moisture than is brought to it, and which therefore are making a salt bed of places that used to be covered by water. In like manner there is evidence that the Great American Lakes formerly had a drainage with the Gulf of Mexico. Steamers have been actually known, in former years and in times of freshets, to pass from the Mississippi over into the lakes. At low water, the dry bed of a river can be traced between them. Now the Salt lake of Utah is to the southward and westward of our northern lake basin : that is the quarter whence the rain winds have been supposed to come. May not the same cause which lessened the precipitation or increased the evaporation in the Salt lake water-shed, have

done the same for the water-shed of the Great American system of lakes?

If the mountains to the west, the Sierra Nevada, stand higher now than they formerly did; and if the winds which fed the Salt lake valley with precipitation had, as I suppose they have, to pass the summits of these mountains, it is easy to perceive why the winds should not convey as much vapor across them now, as they did when the summit of the ranges was lower and not so cool.

The Andes, in the trade-wind region of South America, stand up so high that the wind, in order to cross them, has to part with all its moisture; and consequently there is, on the other side, a rainless region. Now suppose a range of such mountains as these to be elevated across the track of the winds which supply the Lake country with rains: it is easy to perceive how the whole country watered by the vapor which such winds bring, would be converted into a rainless region.

I have used these hypothetical cases to illustrate a position which any philosopher, who considers the geological agency of the winds, may with propriety consult, when he is told of an inland basin, the water level of which it is evident was once higher than it now is; and that position is, that though the evidences of a higher water-level be unmistakable and conclusive, it does not follow, *therefore*, that there has been a subsidence of the lake basin itself, or an upheaval of the water-shed drained by it.

The cause which has produced this change of water-level, instead of being local and near, may be remote: it may have its seat in the obstructions which have been interposed in some other quarter of the world; which obstructions may prevent the winds from taking up, or from bearing off, their wanted supplies of moisture for the region whose water-level has been lowered.

I am not prepared to maintain that the water-level of our great system of lakes has been changed by any such process; though I do not think it improbable. Nor am I prepared to ascribe the change of Utah lake *wholly* to obstructions, near or remote, which have prevented the winds from bringing as many and as copious rain-clouds as they at some remote period were wont to bring to this valley; though in this case it appears obvious that the precipitation has diminished, and the evaporation has increased; and it is not easily perceived how a mere subsidence of the Lake basin would change the rate of evaporation, or alter the amount of precipitation there.

Having, therefore, I hope, made clear the meaning of the question proposed, by showing the manner in which winds may become important geological agents; and having explained how the upheaving of a mountain range in one part of the world may, through the winds, affect climates and produce geological phenomena in another, I return to the Dead Sea, and the great inland basins of Asia, and ask, how far it is possible for the elevation of the South American continent, and the upheaval of its mountains, to have had any effect upon the water-level of these seas? There are indications that they all once had a higher water-level than they now have; and that formerly the amount of precipitation was greater than it now is: then what has become of the sources of vapor?

A chain of evidence, which it would be difficult to set aside, can be introduced, if required, to show that the vapor which supplies the extra-tropical regions of the North with rains, comes, in all probability, from the trade-wind regions of the southern hemisphere.

The prevailing winds of the temperate zones blow towards the poles: they are going from warmer to colder climates. Consequently their capacity for moisture is decreased, with their temperature; and they must precipitate, in their way from warmer to colder regions, more water than they can take up again.

The prevailing winds of the torrid zone blow towards the equator: they are going from colder to warmer climates. Their capacity for moisture is therefore on the increase; and they therefore must evaporate, from this zone, more water than they precipitate upon it again.

All the great rivers lie in the northern hemisphere. With more land and less water, its total amount of precipitation is nevertheless greater than that of the southern hemisphere.

The evaporating surface of sea water exposed to the action of the southeast trade winds exceeds, several times in extent, that upon which the northeast trade winds are known to play. These southeast trade winds, when they arrive at the belt of equatorial calms, charged with vapor from the sea, should, when they rise up and come over into this hemisphere, take, in consequence of the earth's diurnal motion, a direction to the northeast. This is the direction which the rains of the Mississippi valley indicate, and which the microscope of Ehrenberg has proved that the southeast trade winds do take: for in a northeasterly direction from the great river basins

of equatorial America, and in the vicinity of the Cape de Verd islands; at Lyons and Genoa; in Malta and the Tyrol; showers of the so-called scirocco dust are known to occur. That celebrated microscopist has examined, with the utmost care, specimens of this dust; and in every specimen that has come to his notice during a period of sixteen years, he has recognized the same organisms, the same forms in them all; and he traces the *locus* of the great majority of them to the trade-wind regions of South America.

Now, if it be true that the trade winds from that part of the world take up there the water which is to be rained in the extra-tropical north, the path ascribed to the southeast trades of Africa and America, after they descend and become the prevailing southwest winds of the northern hemisphere, should pass over a region of less precipitation, generally, than they would do, if, while performing the office of southeast trades, they had blown over water instead of land. The southeast trade winds, with their load of vapor, whether great or small, take, after ascending in the equatorial calms, a northeasterly direction : they continue to flow in the upper regions of the air, in that direction, until they cross the tropic of cancer. The places of least rain, then, between this tropic and the pole, should be precisely those places which depend for their rains upon the vapor which the winds that blow over southeast trade-wind Africa and America, convey.

Now, if we can trace the path of these winds through the extra-tropical regions of the northern hemisphere, we shall be able to identify it by the footprints of the clouds; for the paths of the winds which depend for their moisture upon such sources of supply as the dry land of Central South America and Africa, cannot lie through a country that is watered well.

It is a remarkable coincidence, at least, that the countries in the extra-tropical regions of the north, that are situated to the northeast of the southeast trade winds of South Africa and America; that the countries with us, over which theory makes these winds to blow, include all the great deserts of Asia, and the districts of least precipitation in Europe*.

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* Let any one take a map of Mercator's projection, and on it draw lines from the tropic of cancer towards the north, to represent the probable route and direction which the trade winds of the two southern continents take, in their general channels of circulation over the northern continents. The country between these

The hyetographic map of Europe, in Johnston's beautiful Physical Atlas, places the region of least precipitation between these two lines.

It would seem that nature, as if to reclaim this "lee" land from the desert, had stationed by the wayside of these winds a succession of inland seas to serve them as a line of relays, for supplying with moisture this thirsty air. There is the Mediterranean Sea, the Caspian Sea, and the Sea of Aral, all of which are situated exactly in this direction; as though these sheets of water were designed, in the grand system of aqueous arrangements, to supply with fresh vapor, winds that had already left enough behind them to make an Amazon and an Orinoco of.

The Andes were once covered by the sea; for their tops are now crowned with the remains of marine animals. When they and their continent were submerged—admitting that Europe in general outline was then as it now is—it cannot be supposed, if the circulation of vapor was then such as I suppose it now to be, that the climates of that part of the old world which is under the lee of those mountains, were then as scantily supplied with moisture as they now are. When the sea covered South America, the winds had nearly all the waters, which now make the Amazon, to bring away, and to distribute among the countries situated along the route ascribed to them.

Is there any evidence that the basin which holds the Caspian sea has been more copiously watered than it is now? There is evidence in favor of the probability that it has been; for portions of that sea have retired, and left salt beds behind.

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two lines is the country which, in the general system of atmospherical circulation, lies under the lee of southeast trade-wind Africa and America. And to see where this country is, we have first to ascertain where those two points on the equator are, between which the southeast trade winds cross, after having traversed the greatest extent of land surface in South America; and then from these points to project lines in the direction which these winds are supposed to take, after rising up in the equatorial calms. These two points will be, one near the mouth of the Amazon, the other not far from the Gallapagos islands: the part of the equator between them is the part crossed by the southeast trades, after having traversed the greatest extent of land from whose surface the supplies of moisture are most scanty. A line from the Gallapagos through Florence in Italy, and another from the mouth of the Amazon through Aleppo in Holy Land, would, after passing the tropic of cancer, mark upon the surface of the earth the route of these winds: this is that "lee country," which, if such be the system of atmospherical circulation, ought to be scantily supplied with rains.

If ever the Caspian sea exposed a larger surface for evaporation than it now does; if the precipitation in that valley ever exceeded the evaporation from it, as it does in all valleys drained into the open sea; then there must have been a change of hygrometrical conditions there. And admitting the vapor-springs for that valley to be situated in the direction supposed, the rising up of a continent from the bottom of the sea, or the upheaval of a range of mountains across their route in certain parts of America, Africa or Spain, might have been sufficient to rob the air of the moisture which it was wont to carry away and precipitate upon this great inland basin. See how the Andes have made Atacama a desert, and of Western Peru a rainless country, simply by the rising of a mountain range between these regions and their vapor-springs.

That part of Asia, then, which is under the lee of southeast trade-wind Africa, lies to the north of the tropic of cancer, and between two lines, the one passing through Cape Palmas and Medina, the other through Aden and Delhi. Being extended to the equator, they will include that part of it which is crossed by the continental southeast trade winds of Africa, after they have traversed the greatest extent of land surface.

The range which lies between the two lines that represent the course of the American winds and vapors, and the two lines which represent the course of the African winds and vapors, is the range which is under the lee of winds that have for the most part traversed water surface, or the ocean, in their circuit as southeast trade winds. But a bare inspection of the chart will show that the southeast trade winds which cross the equator between long. 15° and 50° W., and which are supposed to blow over into this hemisphere between these two ranges, have traversed land as well as water; and that it is precisely those winds, which in the summer and fall are converted into southwest monsoons for supplying the whole extent of Guinea with rains and rivers. Those winds, therefore, it would seem, leave much of their moisture behind them, and pass along to their channels in the grand system of circulation, for the most part as dry winds. Moreover, it is not to be supposed that the channels through which the winds that cross the equator at the several places named, are as sharply defined in nature as the lines suggested would represent them to be.

The whole region of the extra-tropical old world, that is included within the ranges marked, is the region which has most land to wind-

ward of it in the southern hemisphere. Now it is curious that all the great extra-tropical deserts of the earth, with those regions in Europe and Asia which have the least amount of precipitation upon them, should lie within this range. That they are situated under the lee of the southern continents, and have but little rain, may be a coincidence, I admit; but that these deserts of the old world are placed where they are, is no coincidence, no accident : they are placed where they are, and as they are, by design; and in being so placed, it was intended that they should subserve some grand purpose in the terrestrial economy. Let us see, therefore, if we can discover any marks of that design—any of the purposes of such an arrangement—and trace any connection between that arrangement and the supposition which I maintain, as to the place whence the winds that blow over those regions derive their vapors.

It will be remarked at once, that all the inland seas of Asia, and all those of Europe, except the semi-freshwater gulfs of the north, are within this range. The Persian Gulf and the Red Sea, the Mediterranean, the Black and the Caspian, all fall within it. And why are they planted there? Why are they arranged to the northeast and southwest under this lee, and in the very direction in which theory makes this breadth of thirsty winds to prevail? Clearly and obviously, one of the purposes in the Divine economy was, that they might replenish with vapor the winds which are almost vaporless when they arrive at these regions in the general system of circulation. And why should these winds be almost vaporless; but that, when in the general system of circulation they come to the place for taking up vapor, the needful supplies are not to be had; or being obtained, have since been taken away by the cool tops of mountain ranges over which these winds have had to pass.

In the Mediterranean, the evaporation is greater than the precipitation. Upon the Red Sea, there never falls a drop of rain : it is all evaporation. Are we not therefore entitled to regard the Red Sea as a make-weight thrown in to regulate the proportion of cloud and sunshine, and to dispense rain to certain parts of the earth in due season and in proper quantities? Have we not, in these two facts, evidence conclusive that the winds which blow over these two seas come, for the most part, from a dry country, from regions which contain few or no pools to furnish supplies of vapor?

Indeed, so scantily supplied with vapor are the winds which pass in the general channels of circulation over the water-shed and sea-

basin of the Mediterranean, that they take up there more water as vapor than they deposit. Throwing out of the question what is taken up from the surface of the Mediterranean itself, these winds deposit more water on the water-shed of that sea than they take up from it again. The excess is to be found in the rivers which discharge into the Mediterranean; but so thirsty are the winds which blow across the bosom of that sea, that they not only take up again all the water that those rivers pour into it, but they create a demand for an immense current from the Atlantic to supply the rest.

It is estimated that three* times as much water as the Mediterranean receives from its rivers, is evaporated from it. This may be an over-estimate; but the fact is made obvious by the current which the Atlantic sends in through the Straits of Gibraltar, that the evaporation from it is in excess of the precipitation; and that the difference, whether it be much or little, is carried off to modify climate elsewhere; to refresh with showers, and make fruitful, some other parts of the earth.

The great inland basin of Asia in which are Aral and the Caspian seas is situated on the route which I make these thirsty winds from southeast trade-wind Africa and America to take; and so scant of vapor are these winds when they arrive in this basin, that they have no moisture to leave behind: just as much as they pour down, they take up again and carry off. The level of the Caspian sea is as permanent as that of the whole ocean. We know that the volume of water returned by the winds, the rains, and the dews, into the whole ocean, is exactly equal to the volume which those seas give back to the atmosphere; for as far as our knowledge extends, the level of each of these two seas is as permanent as that of the great ocean itself.

These winds, therefore, do not begin permanently to lay down their load of moisture, be it great or small, until they cross the Oural mountains. On the steppes of Issim we find them first beginning to lay down more than they take up again, after they have supplied the Amazon and the other great equatorial rivers of the South. In the Obi, the Yenesi and the Lena, is to be found the volume which contains the expression for the load of water which these winds have brought from the southern hemisphere, the Medi-

* *Vide* Article "Physical Geography," Encyclopædia Britannica.

terranean and the Red sea; for in these almost hyperborean river-basins do we find the first instance in which, throughout the entire range assigned these winds, they have, after supplying the Amazon, &c., left more water behind them than they have taken up again and carried off. The low temperatures of Siberian Asia are quite sufficient to extract from these winds the remnants of vapor which the cool mountain tops and mighty rivers of the southern hemisphere have left in them.

Here I may be permitted to pause, that I may call attention to the remarkable coincidence, and admire the marks of design, the beautiful and exquisite adjustments that we see here provided, to ensure the perfect workings of the great Atmospherical Machine. The coincidence is between the hygrometrical conditions of all the countries within, and the hygrometrical conditions of all the countries without, the range included within the lines which I have drawn to represent the route in this hemisphere of the southeast trade winds which have blown their course over the land in South Africa and America. Both to the right and the left of this range, are countries included between the same parallels in which it is; yet these countries all receive more water from the atmosphere, than they give back to it again: they all have rivers running into the sea. On the one hand, there are in Europe the Rhine, the Elbe, and all the great rivers that empty into the Atlantic: on the other hand, there are in Asia, the Ganges, and all the great rivers of China; and in North America, in the latitude of the Caspian sea, is our great system of fresh-water lakes: all of these receive from the atmosphere immense volumes of water, and pour it into the sea in streams the most magnificent.

It is remarkable that none of these copiously supplied water-sheds have to the southwest of them, in the trade-wind regions of the southern hemisphere, any considerahle body of land: they are all of them under the lee of evaporating surfaces, of ocean waters, in the trade-wind region of the south. Only those countries in the extra-tropical north, which I have described as lying under the lee of trade-wind South America and Africa, are scantily supplied with rains.

The surface of the Caspian sea is about equal to that of our lakes: in it, evaporation is just equal to the precipitation. Our lakes are between the same parallels, and about the same distance from the western coast of America that the Caspian is from the western coast

of Europe; and yet the waters discharged by the St. Lawrence give us an idea of how greatly precipitation is in excess of evaporation here with us. To windward of the lakes, and in the trade-wind regions of the southern hemisphere, is no land. Therefore, supposing that such as I maintain is the course of the vapor-distributing winds, ought they not to carry more water from the ocean to the lakes, than from the land—from the interior of South Africa and America—to the Caspian?

In like manner, extra-tropical New-Holland and South Africa have each land—not water—to the windward of them in the trade-wind regions of the northern hemisphere, where the vapor for their rains ought to be taken up: they are both countries of little rain; but extra-tropical South America has, in the trade-wind region to windward of it in the northern hemisphere, a great extent of ocean, and the amount of precipitation in extra-tropical South America is wonderful. The coincidence, therefore, is remarkable, that the countries in the extra-tropical regions of this hemisphere, which lie to the northeast of large districts of land in the trade-wind regions of the other hemisphere, should be scantily supplied with rains; and likewise that those so situated in the extra-tropical south, with regard to land in the trade-wind region of the north.

Having thus remarked upon the coincidence, let us turn to the evidences of design, and contemplate the beautiful harmony displayed in the arrangement of the land and water, as we find them along this conjectural "wind-road."

Those who admit design among terrestrial adaptations, or have studied the economy of cosmical arrangements, will not be loth to grant that the atmosphere keeps in circulation a certain amount of moisture; that the waters of which this moisture is made are supplied by the aqueous surface of the earth, and returned to the seas again through rivers and the process of precipitation; that a permanent increase or decrease of the quantity of water thus put and kept in circulation by the winds would be followed by a corresponding change of hygrometrical conditions, which would draw after it permanent changes of climate. Permanent changes of climate would involve the ultimate well-being of myriads of organisms, both in the vegetable and animal kingdoms.

The quantity of moisture that the atmosphere keeps in circulation is, no doubt, just that quantity which is best suited to the well-being, and for the proper development of the vegetable and animal king-

doms; and that quantity is dependent upon the arrangement that we see in nature between the land and the water — between mountain and desert, river and sea. If the seas and evaporating surfaces were changed, and removed from the places they occupy, to other places, the places of precipitation probably would also be changed: whole families of plants would wither and die for the want of cloud and sunshine, dry and wet, in proper proportions; and, with the blight of plants, whole tribes of animals would also perish, and, under such a chance arrangement, man would no longer be able to rely upon the early and the latter rain, or to count with certainty upon the rains being sent in due season for seed-time and harvest. And that the rain will be sent in due season, we are assured; and when we recollect who it is that "sendeth" it, we feel the conviction strong within us, that He that sendeth the rain has the winds for his messengers; and that they may do his bidding, the land and the sea were arranged, both as to position and proportion, where they are, and as they are.

It should be borne in mind that the southeast trade winds, after they rise up at the equator, have to overleap the northeast trade winds. Consequently they do not touch the earth until near the tropic of cancer — more frequently to the north, than to the south of it; but for a part of every year, the place where these vaulting southeast trades first strike the earth, after leaving the other hemisphere, is very near this tropic. On the equatorial side of it, be it remembered, the northeast trade winds blow: on the polar side, what was the southeast trades, and what is now the prevailing southwesterly winds of our hemisphere, prevail. Now take a map of the Eastern hemisphere, and it will be seen that the upper half of the Red Sea is north of the tropic of cancer; the lower half, to the south of it: that the latter is within the northeast trade-wind region; the former, in the region where the southwest passage winds are the prevailing winds.

The River Tigris is probably evaporated from the upper half of this sea by these winds; while the northeast trade winds take up from the lower half, those vapors which feed the Nile with rain, and which the clouds deliver to the cold demands of the Mountains of the Moon. Thus there are two "wind-roads" crossing this sea: to the windward of it, each wind path is through a rainless region; to the leeward, in each case, is a river to cross.

The Persian Gulf lies for the most in the track of the southwest

winds : to the windward of the Persian Gulf is a desert; to the leeward, the River Indus. This is the way in which theory would require the vapor from the Red Sea and Persian Gulf to be conveyed; and this is the way in which we find indications that it is conveyed. For to leeward do we find, in each case, a river, telling to us by signs not to be mistaken, that it receives more water from the clouds than it gives back to the winds.

Is it not a curious circumstance that the winds which travel the road suggested from the southern hemisphere, should, when they touch the earth on the polar side of the northern tropic, be so thirsty, more thirsty, much more, than those which travel on either side of their path, and which are supposed to have come from southern seas, not from southern lands?

The Mediterranean has to give those winds three times as much vapor as it receives from them : the Red Sea gives them as much as they will take, and receives nothing back in return; the Persian Gulf, doubtless, gives more than it receives. What becomes of the rest? Doubtless it is given to the winds, that they may bear it off to distant regions, and make fruitful, lands that but for these sources of supply would be almost rainless, if not entirely arid, waste and barren.

These seas and arms of the ocean now present themselves to the wind as counterpoises in the great hygrometrical machinery of the earth. As sheets of water placed where they are, to balance the land in the trade-wind region of South America and South Africa, they now present themselves. When the foundations of the earth were laid, we know who it was that "measured the waters in the "hollow of his hand, and meted out the heavens with a span, and "comprehended the dust of the earth in a measure, and weighed "the mountains in scales, and the hills in a balance."

Here then we see harmony in the winds, design in the mountains, order in the sea, arrangement in the dust. Here are signs of beauty and works of grandeur; and we may now fancy, that in this exquisite system of adaptations and compensations, we can almost behold in the Red and Mediterranean seas the very waters that were held in the hollow of the Almighty hand, when He weighed the Andes and balanced the hills in Africa.

In that great inland basin of Asia which holds the Caspian sea, and embraces an area of one million and a half of geographical square miles of land, we see the water-surface so exquisitely ad-

justed, that it is just sufficient, and no more, to return to the atmosphere as vapor, exactly as much moisture as the atmosphere lends in rain to the rivers of that basin.

Thus we may regard the Mediterranean, the Red sea and Persian gulf as relays, distributed along the route of these thirsty winds from the continents of the other hemisphere, to supply them with vapor, or to restore to them that which they have left behind to feed the sources of the Amazon, the Niger, and the Congo.

In contemplating the office of the winds in the distribution of moisture over the earth, we may liken them to messengers that are heavily tasked, being laden with as much as they can bear. The load of water given to them to carry away from the sea into the recesses of the most distant mountains, becomes too heavy, and then it is precipitated as mountain torrents. There is then a change of temperature : the atmosphere is invigorated; and straightway the winds commence to lift up their load again ; taking as before, a large portion of that which they had just let down to rest.

A change occurs in the sublime economy, by which to-day the winds are relieved of their load in one part of the valley of the west : they precipitate and pass on. To-morrow, fresh air arrives ; and it commences straightway to take up this load again—to evaporate from leaf, twig and soil, all the moisture it can find, and to bear it off to make rains for the Lake country or some other land.

The change of temperature from day to day accomplishes important ends in the grand arrangement for giving circulation to moisture, and rains to the earth. According to the beautiful series of observations, which, at my request, a brother officer* conducted upon the habits of the Mississippi river as it passes Memphis in Tennessee, it appears that only about one-sixth of the water that is rained in that valley reaches the ocean through that river. The other five-sixths are taken up again into the air, and are carried off in the general channels of circulation to supply other systems of lake and river basins.

The hypothesis that the winds from South Africa and America do take the course through Europe and Asia which I have marked out for them, is supported by so many coincidences, to say the least, that we are entitled to regard it as probably correct, until a train of

* Robert A. Marr, U. S. N.

coincidences as striking can be adduced to shew that such is not the case.

Returning once more to a consideration of the geological agency of the winds in accounting for the depression of the Dead Sea, we now see the fact most strikingly brought out before us, that if the Straits of Gibraltar were to be barred up, so that no water could pass through them, we should have a great depression of water-level in the Mediterranean. Three times as much water is evaporated from that sea, as is returned to it through the rivers. A portion of water evaporated from it is probably rained down and returned to it through the rivers; but — supposing it to be barred up — as the demand upon it for vapor would exceed the supply by rains and rivers, it would commence to dry up. As it sinks down, the area exposed for evaporation would decrease, and the supplies to the rivers would diminish, until finally there would be established between the evaporation and precipitation an equilibrium, as in the Dead and Caspian seas; but for aught we know, the water-level of the Mediterranean might, before this equilibrium were attained, have reached a stage far below that of the Dead Sea level.

The Lake Tadjura is now in the act of attaining such an equilibrium: there are connected with it the remains of a channel by which the water ran into the sea; its surface is now 500 feet below the sea level, and it is salting up. If not in the Dead Sea, do we not, in the valley of this lake, find outcropping some reason for the question, What have the winds had to do with the phenomena before us?

The winds, in this sense, are geological agents of great power. It is not impossible but that they may afford us the means of comparing, directly, geological events which have taken place in our hemisphere, with geological events in another. The tops of the Andes were once at the bottom of the sea. Which is the oldest formation, that of the Dead Sea, or the Andes? If the former be the older, then the climate of the Dead Sea must have been hygrometrically very different from what it now is.

In regarding the winds as geological agents, we can no longer consider them as the type of instability. We rather behold them in the light of ancient and faithful chroniclers, which, upon being rightly consulted, will reveal to us truths which nature has written upon their wings in characters as legible and as enduring as she ever engraved the history of geological events upon the tablet of the rock.

Prof. RODGERS has suggested the idea that the salt of the sea is washed into it by the rains and rivers from the land. The waters of Lake Titicaca, which receives the drainage of the great inland basin of the Andes, are only brackish, not salt. Hence we may infer that this lake has not been standing long enough to become brine, like the waters of the Dead Sea : consequently it belongs to a more recent period. On the other hand, it will also be interesting to hear that my friend Captain LYNCH informs me, that in his exploration of the Dead Sea, he saw what he took to be the dry bed of a river that once flowed from it. And thus we have two more stout links, and strong, to add to the chain of circumstantial evidence going to sustain the testimony of this strange and fickle witness which I have called up from the sea to testify in this presence concerning the works of nature, and to tell us which be the older, the Andes watching the stars with their hoary heads, or the Dead Sea sleeping upon its cubic beds of crystal salt.

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16. FAULT IN A METALLIC VEIN AS SEEN AT STERLING MINE, NEW-JERSEY. By A. C. FARRINGTON, of Newark, N. J.

IN removing the white limestone from the southeasterly side of the zinc mine at Sterling Hill, at the distance of four feet and nine inches from the vein of red zinc ore, and parallel to the same, was found a vein about seven inches wide, in the white limestone, that exhibits a striking example of the kind of displacement called *fault*. The metallic vein is one composed of sulphuret of zinc, copper and galena. The vein has been removed at the fracture 4½ inches. A trace like that of a blacklead pencil connects the several parts, and the displacement took place under circumstances highly favorable for the limestone becoming again united : it is so completely solidified, that, from texture or fracture, no evidence can be obtained of a former displacement.

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17. ON THE SILT AND DRAINAGE OF THE MISSISSIPPI RIVER. By Lieut. M. F. MAURY, U. S. N.

[*Not received.*]

18. On the Boulder Hypothesis. By Hon. A. Osborne, of Albany.

[*Not received.*]

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19. On a distorted Quartz Vein in Sienite. By A. C. Farrington, of Newark.

[*Not received.*]

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20. On the Methods of Investigation adopted in the Geological Survey of Pennsylvania. By Prof. H. D. Rogers.

[*Not received.*]

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21. On the Origin of Stratification. By D. A. Wells, Esq., of Cambridge.

The general idea respecting the origin or cause of stratification, as expressed in geological text-books, or as inferred from the writings of geologists, seems to be this : that strata, or the so-called divisions of sedimentary matter, have been produced either by an interruption of deposition, or a change in the quality of the material deposited. This idea is well illustrated by the deposition of matter by tides or inundations, its subsequent consolidation, and a renewed deposition on the plane of the former deposit. That such is really the cause of stratification in many cases, I do not dispute; but that there are other causes which tend to produce and have produced stratification, equally extensive and varied, is, I think, clearly shown by the following observations :

My attention was first drawn to the subject during the past summer, while engaged in the analysis of soils. By the process adopted, the soil was washed upon a filter for a considerable number of days, in some cases for a period as long as two weeks, and subsequently dried at a temperature of 250° F. The residue of the soil left upon the filter, consisting chiefly of silica and alumina, was found, after drying, in every instance, to be more or less stratified, and that too by

divisional planes, in some cases not at all coincident with any division of the materials, although this is apt to take place. The strata so produced were in some instances exceedingly perfect and beautiful, not altogether horizontal, but slightly curved, and in some degree conforming to the shape of the funnel. The production of laminæ was also noticed, especially by the cleavage of the strata produced, into delicate, thin, parallel plates, when moistened with water. These arrangements, it is evident, were not caused by any interruption and renewal of the matter deposited, or by any change in the quality of the particles deposited, but from two other causes entirely distinct, and which I conceive to be these: first, from a tendency in earthy matter, subjected to the filtering, soaking, and washing of water for a considerable period, to arrange itself according to its degree of fineness, and thus form strata; and secondly, from a tendency in earthy matter, consolidated both by water and subsequent exsiccation, to divide, independently of the fineness or quality of its component particles, into strata and laminæ. The tendency of this earthy matter is generally to divide along the lines formed by the arrangement of the particles according to their nature or quality: this is not, however, always the case, as was proved by the observations noted, and which is also conclusively shown by the examination of almost any stratified rocks.

In the valley of the Connecticut, where the sandstones remain unaltered in any great degree by heat or dislocation, the stratification produced by the several causes may be clearly seen and studied. On the western edge of this deposit, we have rocks composed of strata, which would at once be referred to the action of tides or inundations by the most inexperienced observer. The strata here vary from a tenth of an inch to an inch in thickness; they are also covered with mud-cracks, and the various markings which are usually found upon a shore or beach. In other portions of the valley, we have strata divisions occasioned by the lines which separate materials differing either in quality or nature, as in the shales from the sandstone, the coarse conglomerates from the fine sandstone, or the highly bituminous shales from those less bituminous. And then upon the extreme eastern edge of this sandstone deposit, we find strata, the leaves of which measure from one to two, and, in some instances, three feet in thickness, each embracing in itself matter ranging from a coarse conglomerate to the finest sand; and yet none of these, within the limits of the particular strata in which they are included, exhibit the

slightest tendency to break or divide in any one direction more than another.

The observations here stated, I am happy to find, have been also noticed to some extent by others conversant with the subject of stratification. Sawdust, subjected to the filtering action of water, has been observed by Prof. AGASSIZ to assume a regular stratified appearance. The same has also been noticed by Dr. HAYES of Boston, in the vats into which clay, used for the manufacture of alum, is washed. I have also noticed regular stratification in the dried deposit of a puddle in the streets, where no apparent change in the character of the materials deposited could be noticed, and when there was certainly no interruption of deposition.

If the divisions of stratification which I have thus pointed out be admitted, it is not improbable that many cases of what are now considered disturbed and tilted strata are none other than their normal condition.

Dr. EMMONS remarked that he agreed entirely with the views brought forward by Mr. WELLS, and referred to cases of clay beds, in which certain strata were contorted and inclined, apparently from forces acting laterally, or from below; but which forces, from the undisturbed condition of the surrounding beds, could not have acted in such a manner as to have produced the disturbance referred to: they must therefore be accounted for by peculiarities or changes in the method of deposition, and by subsequent changes.

Professor HALL stated that he had also accumulated considerable evidence in regard to this subject, and regarded it as highly important in a geological point of view.

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22. ON THE GEOLOGICAL AGE OF THE CLAY SLATE OF THE CONNECTICUT VALLEY, IN MASSACHUSETTS AND VERMONT. By Pres. E. HITCHCOCK, of Amherst College.

THE clay slate deposit of the Connecticut valley, commencing at Bernardston in Massachusetts, and extending northerly into Guilford, Dummerston, &c. in Vermont, has usually been regarded, both from its associations with mica slate, and the absence in it of organic remains, as well as the shining aspect of its surfaces, to belong to a

very ancient date; scarcely newer, indeed, than mica slate. In Bernardston I had long ago found and described encrinal remains in a bed of limestone, which I had supposed to lie upon the slate; but recently I have discovered, I think, a mass of the slate beneath which the limestone passes, although the actual contact be not visible. This fact has led me to look more carefully for the fossils; and I find at least two species of encrinites. These were shown to James Hall, Esq., and he is of opinion that they probably belong to the period of the Onondaga limestone of the New-York Surveys; at least, that they are not more ancient than that rock. Thus we fix the age of this clay slate as a part of the Devonian system, unless there is some mistake in the observations or the opinions as to their character.

If this result be admitted, it does not follow that all the rocks of the Green and White mountains are no older than fossiliferous rocks, as some maintain; for the slate formation in the Connecticut valley is manifestly a newer rock than those which succeed, either on the east or the west. Perhaps it is a portion of the Hudson-river slate, which once arched over the intervening Hoosic mountain, and which has been subsequently worn away except in this deep valley. At any rate, I have met nowhere, either in this country, Great Britain, or Switzerland, with rocks more thoroughly crystalline than those which constitute most of the White mountain ranges. Gneiss, mica slate, and hornblende slate, just such as you find in the central Alps in the vicinity of Mont Blanc, constitute these ranges. They may, however, be as new as the palæozoic strata; but if they are, then I think no rocks on the globe probably are older. Such a conclusion I am perfectly ready to admit, when fairly proved; but we should be cautious in admitting a conclusion which goes to the very extreme of metamorphism, without decisive evidence. We should at least wait till the White mountains have been more carefully studied. Years of examination by the ablest geologists will, in my opinion, be requisite, before we understand fully the characters of those mighty ranges. The work has yet scarcely been begun.

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23. On the Geological Age of the Coal-bearing Rocks of North-Carolina. By Prof. W. B. Rogers, of the University of Virginia.

[*Not received.*]

III. PALÆONTOLOGY.

24. Remarks on the Trilobite of the Potsdam Sandstone, named by Dr. Owen *Dikellacephalus*, and its Relations to Asaphus and Ogygia. By Prof. James Hall.

[*Not received.*]

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25. On the Alternations of Marine and Terrestrial Organic Remains in the Carboniferous Series of Ohio. By J. W. Foster.

Mr. Foster commenced by sketching the boundaries and principal groups of rocks in Ohio, and proceeded to show that the equivalents of most of those forming the Silurian and Devonian series were to be found in the New-York classification; but, on the other hand, many of the New-York series, particularly the conglomerates, or those rocks which were indicative of disturbance of the sea bottom, were wanting in Ohio.

He spoke of the necessity of a subdivision of what has been termed the Cliff limestone, into two distinct groups; a subdivision which was fully warranted by the organic remains. The sandstone beneath the productive Coal measures had hitherto been classed as a single group, under the name of Waverly; but he was satisfied that it was susceptible of at least a threefold division, characterized by well defined zoological differences. The entombed remains, he remarked, were more characteristic of the Carboniferous than the Devonian series.

He next proceeded to describe a section along the National road, through a portion of the Ohio coalfield, consisting of alternations of sandstones, shales, limestones, and seams of coal, together with a siliceous deposit provincially known as buhrstone. While the sandstones and shales were characterized in the main by various types of carboniferous flora, the limestones, the buhr, and in some instances the shales, were filled with types exclusively marine. In a vertical

range of about 700 feet, he had been able to recognize no less than ten alternations of marine and terrestrial organic remains.

He next proceeded to describe, somewhat in detail, the character, range, and extent of the associated fossils. The upper portion of the Sub-carboniferous sandstone was characterized by numerous remains of *Producta* (some of the species ranging high into the true Coal series), *Encrinites*, *Phillipsia*, *Spirifer*, and *Lutraria*.

The Buhrstone, although it attained a thickness of about ten feet, was replete with various forms of organic life, some of which were confined exclusively to it. Among them he recognized *Streptelasma*, *Plactostylis*, *Phragmoceras*, *Spirifer*, *Producta*, *Terebratula*, *Allorisma*, *Entrochites*, *Fenestella*, and *Polypora*.

The Cherty limestone, in close proximity, contained *Lutraria*, *Cypricardia*, *Producta*, *Spirifer*, *Chonetes*, *Orbicula*, *Atrypa*, *Littorina*, *Plactostylis*, *Cyathophyllum*, and *Entrochites*.

The next succeeding bed, exclusively wrought in the vicinity of Zanesville, afforded the *Spirifer*, *Producta*, *Atrypa*, *Chonetes*, *Pecten*, *Terebratula*, *Entrochites*, *Polypora*, and *Fenestella*. From it had also been obtained a fish spine, which, according to AGASSIZ, was a new type, intermediate between *Ctenocanthus*, *Gyracanthus* and *Hybodus*.

Ascending in the series, there were three or four beds of limestone in which the traces of organic remains were by no means abundant, but sufficiently so as to leave no doubt of their marine origin. A few miles west of Cambridge, however, there was a bed which was highly fossiliferous, yielding the following genera: *Goniatites*, *Atrypa*, *Spirifer*, *Producta*, *Chonetes*, *Terebratula*, and the teeth of fishes, which, according to AGASSIZ, belonged to the genus *Petalodus*, a form allied to the Port Jackson shark.

Mr. FOSTER remarked, that specimens of these teeth had been shown him by Dr. DELAMATER several years ago; and that within the past season, Prof. SAFFORD had procured a perfect specimen which he exhibited to the section. This was the first instance observed of the occurrence of fish remains in the Coal measures of the United States, and ought to put geologists on their guard against hastily inferring that they did not exist in other deposits.

He next proceeded to remark on the distribution of the marine fauna. While some of the forms, such as the *Producta sulcata*, *Spirifer glaber*, *Spirifer trigonalis*, *Chonetes*, *Terebratula*, and *Entrotrochites lævis* had a wide vertical range, some of them extending through the whole section; others, for example the *Streptelasma*,

the *Phragmoceras* and the *Allorisma,* did not range much above the Buhr.

The flora of this epoch was next described; among which, he had been able to recognize the following genera: *Stigmaria, Sphenophyllum, Annularia, Asterophyllites, Bechera, Equisetum, Calamites, Cyclopteris, Neuropteris, Tæniopteris?, Pecopteris, Lepidodendron, Cardiocarpon, Sigillaria* and *Favularia,* and others belonging to undetermined genera. The fossil flora afforded a less certain guide in the identification of strata, than the fossil fauna; a fact which may be accounted for, on the supposition that the atmosphere which encircled the earth during this epoch was subjected to fewer disturbances than the sea.

He next spoke of the distribution of the Coal plants. The *Stigmaria ficoides* had a wide vertical range, occurring for the most part at the base of the Coal series, and often in a vertical position. Many forms of the *Pecopteris,* particularly one allied to *P. serlii,* were widely distributed. A *Lepidodendron* approaching *L. aculeatum,* forming stems nearly two feet in diameter, occurred in the sandstone at Zanesville. Other plants were very restricted in their range; for example, the *Neuropteris grangeri* had been thus far found only in one locality at Zanesville. He spoke of this region as forming a vast herbarium of the plants of this epoch, not shattered and abraded, but every fibre and foliation perfectly preserved. From their perfect state of preservation, and the comminuted materials in which they were enclosed, as well as the erect position in many instances of the *Sigillariæ,* he inferred that the interval between the severing of the leaf from the parent stalk and its entombment in the clayey paste must have been brief, and attended with no violent agitations of the water equivalent to breaker action, or strong-moving currents. While the forms of organic life associated with the limestones are exclusively marine, and those of the sandstones terrestrial, it frequently happened that both were confusedly mingled in the shales succeeding the former.

Mr. Foster here exhibited a section near Zanesville, where the remains of molluscs and corals were mingled with the delicate fronds of the *Neuropteris* and the seedvessels of flowerless plants.

In conclusion, he adverted to the probability of the proximity of large tracts of land at this period, and of its repeated submergence, or unequal tilting, by which the sea was enabled to extend its dominion over large areas, and the drainage of the subaërial portions

was changed. In no other way could we satisfactorily explain the mingling of terrestrial and marine life. This submergence, it is inferred, was gradual, since there were no conglomerate bands above the base of the coal. The cross-stratification conspicuously displayed in the sandstones, and the coarse grains constituting the mass, would point to the existence of oceanic currents recurring at frequent intervals.

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26. Remarks upon the Fossils of the Potsdam Sandstone. By Prof. James Hall.

[*Not received.*]

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27. On the Vegetation of the Infra-carboniferous Rocks of Pennsylvania, and a Description of a New Genus of Fossil Plants. By Prof. H. D. Rogers.

[*Not received.*]

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28. On some Fossils of Northern Ohio. By Professor J. Brainerd, of Cleveland.

To Prof. Louis Agassiz, President of the American Association for the Advancement of Science.— Albany Meeting, August 1851.

DEAR SIR :

Permit me to call the attention of those members of the Association who feel an interest in the subject of geology, to the group of fossils herewith presented for examination. The locality from which these specimens were taken is one of considerable interest, from the fact that a number of *fossil fishes*, supposed to belong to a new species, have there been obtained.

The geographical location is in the southeast part of Cuyahoga county, Ohio, at the village of Chagrin-Falls, and about eighteen miles southeast of Cleveland. The elevation above Lake Erie is

about three hundred feet. The geological position may perhaps be best defined, by reference to the extensive formation of conglomerate (quartz pebble-rock) which underlies the coalfields of Ohio. This extensive landmark in geology is found cropping out in a bold bluff, to the northeast, east and southeast, a mile or two distant, and is about two hundred feet higher in the series than the stratum which yields these fossils.

The intermediate space between the fossiliferous bed and the conglomerate is occupied chiefly by black slate like that in the specimens, argillaceous shale, and thin beds of hard fine-grained sandstone. The shale which lies above the position of the accompanying fossils, is, very nearly, if not quite, destitute of organic remains; though some faint traces of fossils, apparently *spines*, of half an inch or more in length, have been discovered near the conglomerate, in the township of Russell, about two miles northeast of Chagrin-Falls.

That portion of the shale which contains the fossils is about two inches in thickness, and lies in immediate contact with a stratum of grindstone grit, in beds of various thickness ranging from half an inch to two feet, generally separated by thin partings of clay. The surface of these beds is generally covered with *ripple-marks*. In one of the partings, the surface, over a large extent, appears to be very finely marked with *rain drops*, specimens of which are herewith presented. Other surfaces are completely covered with a network of markings, similar to those made by soft worms upon the surface of the mud of a drying pool of water.

In this quarry were found the fossil fishes which were presented for examination at the meeting of the Association in May last, at Cincinnati. The dip of the rock at this place is southeast, seventeen inches in one hundred feet.

So far as I can learn, the *Lingula elongata* and *Orbicula* have not been discovered in any other portion of the State, in this formation (black slate); and their presence in this location, being so well marked in regard to geological position, may perhaps aid in the classification of the series in Ohio, with those of like formation in the State of New-York.

There is one fossil in the accompanying group, to which I particularly desire to call attention. This may be familiar to some, though I have never had the good fortune to see one from any other locality. Those which I have examined are quite uniform

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in size, varying from half an inch to three-quarters or an inch in length, and from one-quarter to three-eighths of an inch in width, and gradually tapering to a point. A longitudinal line or depression extends the whole length, dividing the body into two equal lobes, not unlike those in the trilobite. The body of the fossil is *ribbed*, with slightly curved elevations and depressions, having the concavity of the line towards the smaller part of the body.

From a careful examination, I have been thus far unable to detect any other parts than those here described.

The partings of the shale, where these fossils are found, seem to indicate that the animal to which it belonged possessed a shelly covering of very delicate texture, similar to that of the *Lingula elongata* and *Orbicula*, which were its living companions.

CLEVELAND, August 16, 1851. JEHU BRAINERD.

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29. REMARKS UPON THE FOSSIL CORALS OF THE GENUS FAVOSITES, AND ALLIED FOSSIL GENERA FAVISTELLA, ASTIOCERIUM, AND OTHERS. By Prof. JAMES HALL.

[*Not received.*]

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30. ON SOME REPTILIAN FOOTMARKS OF THE INFRA-CARBONIFEROUS RED SHALE OF PENNSYLVANIA. By Prof. H. D. ROGERS.

[*Not received.*]

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31. ON THE PALÆOZOIC GENERA TREMATOPORA, CELLEPORA, &c. By Prof. JAMES HALL.

[*Not received.*]

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32. TRACKS, TRAILS, &c. IN THE SHALES AND SANDSTONES OF THE CLINTON GROUP FROM GREEN BAY; WITH REMARKS ON THE THINNING OUT AND REAPPEARING OF THIS PORTION OF THE CLINTON GROUP. By Prof. JAMES HALL.

[*Not received.*]

D. NATURAL HISTORY, INCLUDING PHYSIOLOGY.

I. BOTANY.

1. On two New Species of Juglans. By Dr. John Torrey.

[*Not received.*]

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2. On the Chenopodiaceæ of North America. By Dr. John Torrey.

[*Not received.*]

II. ZOOLOGY.

3. Points in the Economy of the Seventeen-year Locust (*Cicada septemdecim*), bearing upon the Plural Origin and Special Local Creation of the Species. By Dr. W. J. Burnett, of Boston.

The more we investigate the essential structures of animals, and the more extensive our knowledge of their habits and conditions of being, the more does one become impressed with the close relations existing between them and their outward conditions of life. Our faith in an adaptability of animals generally to the external agencies of the world, is lessened; while our belief that the unison of their lives with these agencies is *not* with them a matter of experience, is strengthened.

A careful analysis of these conditions has led some to believe in the special creation of the separate faunas in the localities in which they are found. In a comprehensive article published some time

since, Prof. Agassiz has traced the various phases under which this question may be considered; and in it may be found excellent reasons for the particular creation of each fauna, in its immutability through any period of time. A question allied to this, but based upon a different, and, perhaps, more enlarged view of life, is the one of the primitive numbers of each species. In this we call to our aid embryology, and its allied branches; but the influences which civilization has wrought, both directly and indirectly, upon the ratio of mortality of animal life, affect much the validity of our conclusions. Nevertheless the general tenor of all such inquiries is to show that the number of each species must have been pretty near that which we find in its natural and undisturbed state, instead of a single pair, as otherwise viewed. In a locality, the natural relations of which to animal life have not been disturbed by the agencies of man, we have a right to infer that the present existing state of destructive elements of life is a fair expression of the past, and also that the present rate of the mortality of a species is that to which it has been subjected during past times.

I conceive that if we cannot infer this, when we have no evidence of any changes in the general economy of nature at that locality, we cannot infer anything of the zoological condition of the past. Besides, the comparatively limited term of human experience justifies such view, which has, also, a still more worthy ground of support in the comparison of the animal's fecundity with its natural liabilities of mortality. If, in a term of human experience of one hundred or a thousand years, the natural prolicity of any well-known species only keeps pace with its relative mortality, so that the number of that species at the end of that time is about the same, it is very difficult to comprehend how, even with species of limited numbers, the same power of prolicity could enable a single pair to reach the present numbers under any existing climate of the earth.

Were it so, we should expect to find a very correct ratio subsisting between the present members of any undisturbed species, and its powers of reproduction. But since attention has been called to the subject, and, with many of the lower animals, the ova counted, not only is there no reason for supposing that such relation is present, but in many instances the very opposite is true; a fact, of the the truth of which I have lately been the more and more convinced, from counting the ova of many insects, and comparing the result with their well-known habits and conditions of life.

The value, however, of such zoological phenomena, bearing as they do against the ordinary opinions of the primitive condition of animal life, strike different minds with different force, according to the strength and tenure which their preconceived views may have on their minds. There are examples in which their appears no escape from conclusions of this character; and although I might detail many taken from the ranks of lower animals, yet from its well-marked character, and recent occurrence, I select that furnished in the *Seventeen-year Locust*, as the subject of this paper.

The present year (1851) may be noted as containing an episode of insect life of more than ordinary interest and value; for in it has occurred the grand appearance of the locust. The zoological value of such an event, I deem very great; and more especially so, because, since their last appearance, many changes in our views of animal life have taken place.

The regularity and promptness with which this insect appears, at the end of an interval of seventeen years, is well known in science. Justly does it excite our astonishment that the conditions of its economy should be so unique. During the last two or three times of its appearance, its habits and peculiarities of life were quite thoroughly investigated. I need not therefore allude to them, except so far as they touch our subject.

During the last of May, I had the good fortune to witness their grand appearance in the interior of Pennsylvania. They came forth in their usual and almost incredible number, and a fine opportunity was given me to learn something about their conditions of life. The insect appears in its perfect or *imago* condition simply for the preservation of its species : its period of life in this state is, therefore, quite brief. Both male and female go about their functions immediately on escaping the earth, after which they die. Their existence is therefore almost entirely subterranean, and, considering the depth to which they descend, almost as isolated from the agencies of civilization as those of the tenants of the ocean. It appeared evident, from what I saw of their movements, that, unless swept away by violent currents, they remain generally in the locality of their birth; so that the comers of this year may properly be said to be the linear descendants of those which there appeared fifty or more years since. This is important, as to our determining whether or not they really increase in numbers.

I made strict inquiries of several men who had witnessed this

their fourth appearance through the same tract of country, and their replies always were that they did not think their numbers to vary materially either way. Being men of sense and farmers, I thought them able to judge of this matter, since they regard the ravages of this insect with no common eye. We will now look a little to its powers of reproduction.

The female has about 500 eggs, which, from certain relations of the other sex, which I have made out microscopically, are probably all or nearly all fecundated. We have, then, for every two individuals which have appeared this year, a deposit of 500 embryos for the generation to appear seventeen years hence.

Now, from what has just been stated about the uniformity of their numbers each time, it appears that from the liabilities of destruction during the long term of seventeen years, out of these 500 embryos, only two appear certain of life and appearance in their perfect state; that is, just replacing the two parents. The chances of life, therefore, with this insect, are, in round numbers, two in five hundred. This calculation may seem strange to some; but if we reflect, it can scarcely be otherwise; for suppose the chances were double, that is, 4 in 500, then we should have at each time just double the numbers of their last time, which observation has shown to be untrue, and which would augur much evil for the future condition of the vegetable world in the localities of their appearance. Even if their chances were three in five hundred, or half again the original stock, agriculturists would quickly perceive the difference.

To sum up the matter, then, we have here an insect whose economy and conditions of life are so unique that it is almost entirely isolated from human destructive agencies, and which is obliged to deposit 500 chances for the certainty of securing two. The ovaries have been formed with this capacity, and the whole internal economy is of a corresponding character.

From these data, we can draw two valuable conclusions : 1st, The evidences of design in nature, in thus balancing numbers against chances of mortality for the preservation of the species; 2dly, The plural origin of this species, instead of from a single pair. In the first, such evidence I regard of the highest zoological character, and quite free from many of those objections belonging to analogous evidence generally. As to the second, it is quite difficult to conceive how the present myriads could have arisen from a single pair, even

if their chances of life were increased 20 or 30 per cent, which we cannot believe possible with the present climate of the earth.

Regarding then, these insects from these data as a special local creation, and whose orignal numbers were nearly as at present, we find the same view supported by different grounds. I refer to the fact of the different years in which they make their appearance in different portions of the country.

Although during the present year, and the past ones divisible by the number 17, have been those of its greatest appearance, yet the appearance of smaller numbers at different years has been noticed in various or even in the same portions of country. In the southern portion of New-England, different parcels have appeared at irregular periods; and in some of the Middle States, there are localities that have four distinct appearances of this insect. Now, as there is no evidence for our thinking that they are ever unfaithful in their time, appearing at the end of a longer or shorter interval than seventeen years, we are forced to the belief of not only their special local creations, but special creation at different periods in the same locality. The ground of such inferences is, I think, equally as tenable as much in geology and palæontology, and certainly is in accordance with many of the recognized principles of zoological science.

I cannot dismiss the subject, without expressing the wish that studies of this kind may be prosecuted in every direction throughout the Animal Kingdom; for thence we can expect results which will be not only new, but serve to illustrate some of the highest and most important relations animals sustain to the surface of our globe.

Prof. Haldeman observed that a period of three years had been ascribed to cicadæ of Europe, but that such a great difference between different species was very unlikely. He suggested the propriety of investigating the period necessary to mature the egg. He then alluded to the difference of the musical organs in different species.

Mr. Goadby inquired if the fecundity of the *C. septemdecim* was considered a peculiarity to meet the contingencies of long existence, and spoke of the general fecundity observed among insects.

Mr. Haldeman said that the fecundity of butterflies was repressed by ichneumonidæ, etc. The *Cicada* had an enemy in the mole, burrowing to the places occupied by the larvæ. He con-

sidered that the irregular appearance of the insect was owing to the overlapping of adjoining districts, and that this irregularity was an argument for the modern origin of the animal, and that these irregularities would finally disappear when the districts overlapped perfectly.

Prof. Agassiz said that the subject opened a new field concerning the introduction of animals, and alluded to the fact that the *Gordius* was obliged to produce 1000000 ova to continue the species.

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4. On the Habits of the Whale. By Lieut. M. F. Maury, U. S. N.

[*Not received.*]

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5. On a New Type of Generation observed among Medusæ. By Prof. L. Agassiz.

[*Not received.*]

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6. Monograph of the Genera Dysnomia (Ag.), Complanaria (Sw.), Lampsilis (Raf.); with General Remarks on the other Genera of Naiades. By Prof. L. Agassiz.

[*Not received.*]

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7. Relations of Embryology and Spermatology to Animal Classification. By Dr. W. J. Burnett, of Boston.

As it is in adult life only that we have the complete maturity of the characteristics which belong to the animal, it is to this stage of existence that we naturally turn to form a basis of a correct animal classification. However sufficient this mode may be for determining the peculiarities of each individual of itself, yet we are constantly

noticing here the want of data, by which the relations of each animal to others can be satisfactorily made out; or, if I may use a figure for expression, I would say that it is necessary to refer to the various paths by which different individuals pursue their course, to learn their natural affinities and relations, which, when they have reached their journey's end, seem much obscured, or to have partially passed away. Within a few years, considerable of this required aid has been found in *embryology*, the details of which have now become sufficient to warrant its consideration in this respect. As will be seen, I have proposed to bring in for aid the value of studies in the counterpart science, *spermatology;* and this aid I hope to show as being both direct and indirect : direct, from its own data; and indirect, as illustrating the error of some impressions we are likely to receive from the pursuit of embryology alone.

The value of embryology here originated with the fact, which we often recognize, viz. the constant succession of types in the animal kingdom; not a succession by transition, but a succession by creation. Like most new doctrines, it has had the fault of being used to cover too much ground; many laboring under the very mistaken idea, that herein might be found the key to all discrepancies in classification.

The greatest benefit of embryology appears to have been in pointing out the four fundamental types of development, and by showing that these are successive, though distinct.

Prof. Agassiz, in a recent course of lectures, has well illustrated this point, and many of its bearings; but upon the details of the subject, we are obliged to put a somewhat varied construction.

In studying the development of embryonic forms, we not unfrequently see peering through the particular form, faint appearances of other types, belonging generally to allied, though sometimes to distant species.

This I cannot better illustrate than by referring to the excellent example Prof. Agassiz has pointed out, which is, that in the early development of a robin, it is first a *palmipede*, and then an *insessorial;* in other words, in this insessorial bird, the palmipede type peers out. Other instances might be quoted, having the same import. Now what construction are we to put upon these phenomena? Shall we say that the palmipede is closely related to the insessorial bird; or that in the successive appearance of the birds upon the earth, the *cleft-footed* sprang out of, or are only a further development of the

web-footed species? Both of these have, I think, a negative answer.

But there is another view of the case, rather more in accordance with our ideas as to the nature of animal types, and which I have also found supported by my spermatological studies : it is, that there has been a succession, by distinct creation, of types in nature; and that wherever these instances occur, those animals, the type of which were shadowed in the developing embryo, existed on the earth before the latter. That is, to again recur to the palmipede type in the embryo of the robin, this fact shows as much as any in zoology can, that palmipedes were created before the cleft-footed birds : for, if I may be permitted the use of the figure, Nature seems to be always retrospective rather than prospective in her works; and in the gradual development of the higher forms, she not unfrequently shows that she has in former times wrought also in lower types; the remembrance, as it were, of the latter, often recurring in the perfect development of the former. This is perhaps the reason why, in embryology, in the development of the lower forms, the higher types are *never* prefigured.

These views are certainly at variance with that of transitional development; and beyond what I have just stated, I do not think they have any signification, but must be regarded as simple facts in zoology. This I hope to more fully illustrate when speaking of another branch of the subject.

But if we are thus sometimes fortunate in embryological studies to learn the lower out of the higher type, and also in recognizing their relations as to time of creation, it is far from being always the case : in fact, something of the opposite may be said to exist; that is, even the particular type of the animal itself does not appear until the very last stages of development. This has been well illustrated in some recent writings of Milne-Edwards on the development of the *Terebellæ;* for in them their distinct specific types seemed buried in one general form, and did not appear until the development was nearly complete. I have noticed the same in many insects : in fact, I think you may take any *genus* of insects, and the specific differences will not be apparent, not only during their oval, but during a large portion of their larval existence. In some instances this will hold even with *families*. But these phenomena belong, I think, to those animals, whose period of development is marked by a succession of, as it were, new births, or a constant series of metamorphoses.

Among the *Medusæ*, more than elsewhere perhaps, have the data furnished by embryology been of service in determining the true relation of forms; for here, lower types in full are so shadowed forth, that at first they might appear only as arrested developments of the higher. In the same category belong the phenomena of "alternation of generations," as shown by STEENSTRUPP and others. But in all these cases we must be positive as to what constitute, in developing forms, typical characteristics; for I think that many errors as to the value of embryological data, have arisen from a misinterpretation in this respect.

We will take for instance the class of insects: we have among them an order *Coleoptera*, which is mandibulate in both its larval and imago conditions; we have also an order *Lepidoptera*, which is mandibulate in its larval, and suctorial in its imago condition. Now from these facts are we to affirm that the order Lepidoptera is in advance of that of the Coleoptera; or rather that the *larval* condition of the former corresponds to that of the *imago* of the latter? In my opinion we cannot, any more than we can refer its application to any other of the mandibulate Articulata; the oral organs alone not forming any permanent basis of classification among the Articulata, as daily experience is constantly showing. We cannot, therefore, regard these organs as entering at all into the composition of the grand type of the animal. Still further will I say, that although these different orders of hexapod insects have been included in a single class from the uniformity of their metamorphotic changes, yet strictly they should be viewed as almost different classes; and in putting any signification upon their embryological results, we can apply them only in the order in which they are found. For instance, in studying the Lepidoptera, there may be gathered data which will serve us in better comprehending the succession of creation of family and other types in this order; but these data have but little value when applied to the Coleoptera, or any of the other orders. We shall soon see that spermatology supports this view.

On the whole, then, the aid furnished by embryology towards a true classification of animals, is in its showing where there has been a *succession* rather than an *affinity* of typical forms; where there has been a series, rather than an affiliation of development. Of course, this restricts its value within that of *classes* or *orders;* and I am of the opinion that any broad generalizations made upon the phenomena thus observed, cannot be valid. If, in examining the paths which

particular species have trodden to reach their perfect condition, we are able to learn much about their natural relations, we cannot be too careful with such knowledge, as to our inferences of the condition of other paths which we have never seen.

In spermatology the data are of a different, and, in my opinion, more reliable character. In embryology, we get our data from forms then in the process of development; and as many of the characteristics of these are often transient, we are liable to error from misinterpretation. In spermatology, on the other hand, the data are derived from a perfectly developed form, and which is the representative of the potential whole of the species. As I have said in another place, it is to the male, what the matured offspring is to both the male and female. The spermatozoon is the most elementary material form in which type characteristics are shadowed forth : it approaches nearer the immaterial type itself, than any other particle of organic matter with which we are acquainted. I have therefore thought that differences in its material shape and form should be entitled to considerable value in determining the *relations* and *affinities* of organised beings; and while it is constantly showing that there have been distinct creations of types, it is of no value in pointing out their relative succession. In these respects, it will be seen that the position it occupies to classification is the *inverse* of that of embryology; and so, by the putting of both of them together, we derive all possible aid in this direction.

But that the matter may be the better comprehended, I will state a few general conclusions at which I have arrived in the study of this subject.

1. The spermatozoon is the, and the only, fecundating particle in the seminal fluid.

2. It is perfectly formed only in animals capable of reproduction; being absent in those very old or very young, and in hybrids.

3. Each species has one form only, which is constant and invariable.

These conclusions will serve as our groundwork. It may be asked, if each species has a particular form, by which it can always be recognized? This question has a twofold answer, both positively and negatively; but mostly negatively, for generally the differences do not extend as far down as species. For nearly two years, I have been at work to learn the determinate value of mere form, size and

shape; in this relation, going through all the available species of a genus or genera, and making careful drawings and measurements of each. One of the conclusions which, from such investigations, I have been able to draw, is, that throughout a genus, the generic type of the spermatozoa does not vary either in mode of formation or physical aspect; that is to say, although in widely separated species there may be difference in mere size, etc., yet the main type remains.

From this fact, I have been led to conclude that when a deviation from the main type is found, the species in which this is found must be separated from the others with which, in classification from other data, it has been placed. I am happy to be able to say, however, that rarely or never do the main characteristics and the peculiarities of the spermatozoa conflict : on the other hand, they form generally a mutual support. Their great value lies not only in showing what may be considered fundamental differences in their separation into genera and species, but also in attesting the complete isolation from each other, of these genera and species; it being impossible that their affinities of structure should have ever been the result of a transitional development.

But there is another fact deserving of mention, before I speak of special forms : it is that the higher we ascend in the scale of development, the more varied are the types; so that among Mammalia we often meet with wide differences of these particles, with different genera of the same family; whereas in corresponding genera of a family among the Invertebrata, the differences are exceedingly slight, and difficult to be perceived. I consider this fact of considerable importance as bearing upon the doctrine of the succession of distinct types of animals, and the intervals of their creation.

My confidence in the truth and value of these essential points I do not find to be otherwise than strengthened by daily experience; so much so, in fact, that I feel justified, in many instances, in displacing some genera and species from positions they have occupied from other data.

[Here followed, in the original paper, illustrations of these doctrines, taken from every department of the animal kingdom. But it has been thought proper to omit here all these special descriptions of spermatic particles, not only because the subject is yet incomplete, but also because I hope soon to publish them elsewhere, with figures; and without which, the typical distinctions here insisted upon would not be recognized.]

Taking now a retrospective view of the whole subject, we see that in spermatology, although there is a tendency towards an uniformity of type throughout the animal kingdom, yet in its grand divisions it has a distinctness of physical character and mode of development only reconcilable with the view that they appeared upon the earth's surface by separate and special *fiats* of the Deity; and not only would this view be entertained in regard to grand divisions, but would extend even to classes and families. It may be asked if there is any evidence how, or in what condition they appeared. The answer would be from data of this kind, that they were created as *adults*. For, did all the animals primitively exist as ova, this would presuppose, beside it, a fecundating power, which we have seen to exist only in the spermatozoa, and which have their origin from a peculiar cell development. Now as it is the material representation of the potential whole of the male, it appears that, from this and its mode of development, it should spring from the male, rather than the male from it; and if we consider the minuteness of their size, there would be many difficulties attending their first fecundation. But if, however, *adults* were created, there would then be the creation of the fully expressed animal type, which would be not only more in accordance with the works of the Deity generally, but also would furnish a permanent basis upon which there could be a constant succession of individuals, and that too from that natural function of tissues impressed upon them when formed.

In studies of this kind we ultimately rest on the confines separating the scientific from the higher world of thought; and if the mind is rejoiced in looking back upon the *realities* of the one, it has much more reason to do so in looking forward to the *anticipations* of the other. If, in pure science, we are to have reason and demonstration, and *not* faith, it is here that the latter takes our hand; and it is a faith, too, not blind, but intellectual; and which is constantly leading us on to the full appreciation of that highest and most stable of mental facts, which is, the existence of a First Great Cause, who laid the foundations of the earth, and by special fiats, at different epochs, created all therein.

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8. On the Classification of Mammalia. By Charles Girard, of Washington.

I.

The limits of the class of Mammalia were not clearly understood by the earlier naturalists. Some groups, which in former times were referred to other classes (as Cetacea and Bats), have successively been brought into it. None, however, originally placed in this class have ever required removal elsewhere. Thus the progressive investigations has always increased the number of the representatives of this class.

At the present day, we may safely say that we know all the essential groups of the class of Mammalia, the actual limits of which are acknowledged by every naturalist. Indeed, we must expect many additional species and genera which time and labor will bring to light, either in a fossil state from various depths in the strata which constitute the solid crust of our globe, or else from its actual surface, and belonging to the living fauna contemporary with the human races. Such additions are not expected to change or modify the boundaries of the class, though they may have some importance in the subdivisions and methodical arrangement of the minor groups.

The division of the class into secondary or minor groups, the relationship and subordination of the latter, have attracted the attention of all general writers on zoology. Almost every one has attempted a classification in accordance with the value attributed to one series of characters, rather than to another.

The most ancient authors seem to have occupied themselves but little with zoological characters : hence the subdivisions which they establish among Mammalia are based upon their mode of life, or the elements in which they live.

Next we see the subdivisions based upon external characters, the most striking being selected, such as the locomotive members.

All this prior to the eighteenth century.

Towards the end of that very century, however, comparative anatomy started as a science; and at the beginning of the nineteenth, it introduced an entirely new method of classification. Systematic zoology underwent a metamorphosis.

The first half of the present century had not yet elapsed, when another science grew up with rapid steps, claiming her share in the

question of the natural classification of the animal kingdom : we allude to embryology. The formation of the young mammal, its genesis, its development prior to the period when it makes its first appearance in the world, if not entirely unveiled yet, are no longer mysterious, and their bearing upon systematic zoology is universally felt.

Palæontological data are not less important in arriving at a natural classification, than those derived from either comparative anatomy or embryology ; and indeed palæontology, comparative anatomy and embryology, hold an equal rank in respect to zoology.

As investigations progress in these fields of researches, new light is daily thrown on some obscure points, and difficult questions are thus elucidated ; but as yet, no methodical arrangement of the class of Mammalia has been universally adopted : there is still as much diversity of opinion, and perhaps even more, at the present time, than in the two past centuries, although as a whole our views on the subject have been improved upon those of our ancestors.

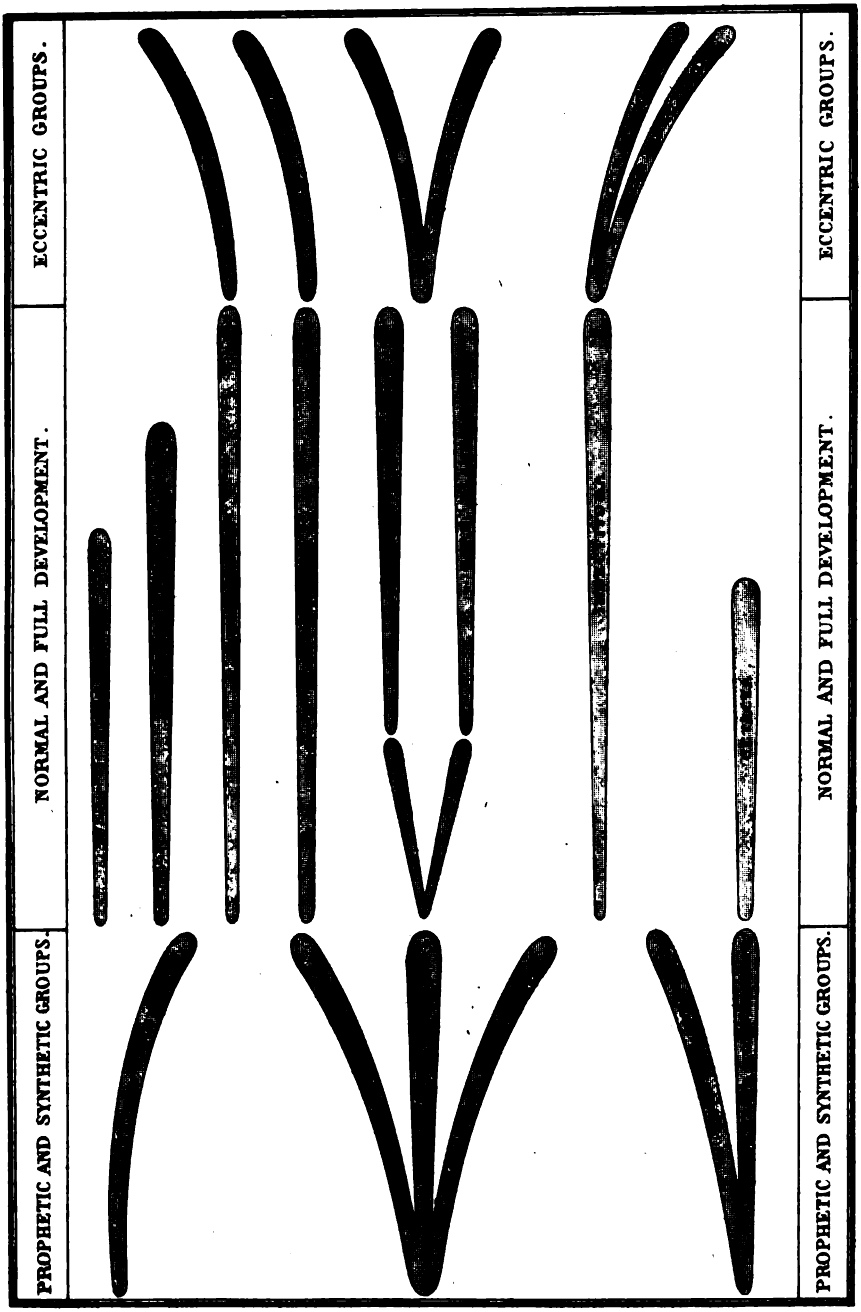
II.

In order to render more tangible our thoughts on the subordination of the various groups which constitute the class of Mammalia, we have prepared the accompanying plate, which we shall now examine.

The orders *Edentata* and *Marsupialia* are considered as the trunks of the class : these two groups, we place on the same level. They constitute the foundation, the bottom of the class, and accordingly are the lowest of all.

The trunk of Edentata sends out three diverging stems, the *Monotremata*, the *Edentata* proper, and the *Tardigrada** : an herbivorous stem (*Tardigrada* s. *Gravigrada*), an insectivorous stem (*Edentata* proper), and a carnivorous stem (*Monotremata*). The carnivorism in the trunk of Edentata is of the lowest grade, and subordinated ; as the carnivorous propensities only attack invertebrates, that is to say, animals of a much inferior rank, comparatively very weak and defenceless.

* The graphic representation on a plane surface has caused the stem of Tardigrada to be separated from its trunk ; but in bringing into contact both edges of the plate, we would obtain a figure similar to that of Marsupialia. Instead of a flattened surface, we want an ideal cone for both trunks.



IDEAL GRADATION OF THE CLASS OF MAMMALIA.

Above Monotremata we place *Cetacea* (whales and dolphins); Edentata proper, above the *Insectivora;* and above Tardigrada, the *Sirenidia,* or so-called herbivorous cetaceans, the *Pachydermata* and *Ruminantia.*

The trunk of Marsupialia exhibits likewise three stems, an *herbivorous,* an *insectivorous* and a *carnivorous.*. Above which we have : the *Rodentia,* continuing the herbivorous stem; the *Insectivora,* continuing the insectivorous stem in common with Edentata proper; and *Carnivora,* continuing the carnivorous stem.

Thus above Edentata and Marsupialia, we have, on another level: *Cetacea, Sirenidia* and *Walrus, Pachydermata, Ruminantia, Rodentia, Insectivora* and *Carnivora;* that is to say, all the normal types which represent the full development of the class as synthetically combined in Edentata and Marsupialia below.

The fact that Insectivora are foreshadowed both by Edentata and Marsupialia, shows that there exists a close connection between the two trunks of the class. The insectivorism is intermediate in rank between herbivorism and carnivorism; it is of a higher grade than the former, and of a lower than the latter. The predominating feature of the trunk of Edentata consists in the vegetable diet, and in the want of a complete set of teeth; the predominating feature of the trunk of Marsupialia, on the contrary, consists in the animal diet, and the possession of a complete set of teeth. Accordingly there can be no doubt that Edentata are lower in grade than Marsupialia : they are the lowest grade in their class.

It will be obvious, also, that here Edentata rank the lowest in grade amongst the normal groups of the class; still showing that Edentata are inferior to Marsupialia, the latter foreshadowing groups of a marked superiority.

Now there are other groups which we place on still another level above the normal types, although not of an absolute superiority. Their place can be nowhere else; their history must follow that of the normal types from which they proceed: the *Bradipodidæ* (or sloths), arising from the herbivorous stem of Edentata; the *Sciuridæ* (or squirrels), arising from the stem of Rodentia; the *Cheiroptera* (or bats), arising from the stem of Insectivora; and the *Quadrumana* (or monkeys), arising from the stem of Carnivora.

We consider these as so many shoots of the mammalian tree, which went beyond the vital sphere of activity of the class; in other words, deviations from the normal development of the class.

III.

§ 1. Let us return now to some of the groups mapped down on our chart of the ideal gradation, and state in a very brief manner their most striking zoological features and relationships.

To begin with *Edentata*, which we concluded were the lowest of the class: when looking at those creatures amidst the other groups, we cannot help being strangely struck by their singular physiognomy, and the still more astonishing association of characters, which appear sometimes rather borrowed from other classes, than as belonging to that of Mammalia. We need only call to mind the water-mole (*Ornithorhynchus*) of New-Holland, the pangolins (*Manis*) of Asia and Africa, the anteater (*Myrmecophaga*) and armadillos (*Dasypus*) of South America, the aard-vark (*Orycteropus*) of the Cape of Good Hope, and the sloths of tropical America, which constitute the three orders *Monotremata*, *Edentata* proper, and *Tardigrada*, the one as strange as the other.

The Monotremata exhibit the lowest grade of mammalian organization. They are ovoviviparous; the young are without uterian connection with the mother, but they are suckled by the latter. In that respect they approach nearest to birds and reptiles; the structure of their sternum and shoulder, also, presents a great resemblance to the same parts in lizards and icthyosauri. Their position at the bottom of the order of Edentata is justified by the fact that one genus (*Echidna*) is completely deprived of teeth, whilst the other (*Ornithorhynchus*) possesses but a few insignificant ones. These two genera, which constitute by themselves the whole order, may just as well constitute two families, so wide are the differences in their general appearance and structure.

The Edentata proper constitute a group exceedingly remarkable, composed of a few genera likewise very strange in their characters, strange in their external features, strange in all their relations. The differences amongst these genera are so great that they have been made the types of as many families by systematic writers, and we believe with great propriety. The absence of teeth is the only character by which they are united, although this character is not absolute, inasmuch as grinding teeth in a very rudimentary state are observed in some few: the front teeth or incisors—those never exist in Edentata. Edentata moreover are provided with strong nails or claws to the four locomotory extremities.

Each of the types in Edentata, by its strange appearance, recals to mind another order of things, another physical period in the earth's history, of which they are mere reminiscences.

The Tardigrada divide into two groups, one completely extinct, the remains of which are found in the tertiary deposits of South America chiefly, the *Tardigrada gravigrada* or *Megatheridæ;* and another exclusively composed of living representatives, the *Tardigrada bradipodida* or sloths of Central and South America.

§ 2. The order *Marsupialia* is another combination into one group of strange forms and strange characters, quite as diversified and heterogeneous as in the Edentata, although Marsupialia seem cast upon a more uniform external mould. The great diversity resides in the physiognomy, and in the structure of the teeth.

In Edentata, we have seen the dentition so defective, that in several cases teeth were entirely absent. Here in Marsupialia the dentition is greatly developed, becomes a permanent character, and requires a contrasting importance. The incisors, it is true, are nowhere six in each jaw, which is the normal number; showing that at the outset the number was of a subordinate value, as well as the relative signification of the different kind of teeth. Nevertheless it can be distinctly shown that the three orders following, Rodentia, Insectivora and Carnivora, are synthetically combined and foreshadowed in the group of Marsupialia, which, when considered zoologically in itself, cannot but strike any one as an odd group standing isolated in the actual creation.

§ 3. The order of *Cetacea*, the lowest amongst the normal groups, may be subdivided into three families. The first and lowest, the family of *Balænidæ*, is characterized by the absence of teeth, or, if not entirely absent, they have no function. These are the *toothless* or *edentated cetaceans*, reminding us of the order of Edentata proper, our second prophetic type. The second family, that of *Physeteridæ*, exhibits well developed teeth on the lower jaw, and rudimentary ones on the upper : the *subdentated cetaceans* of the authors*. The

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* Physeteridæ, or sperm-whales, are more nearly allied to dolphins than to whales, if we take into consideration the structure of the whole skeleton. We might even say that Physeteridæ are gigantic dolphins in which the development of teeth has stopped, and the body increased beyond all proportion. That colossal mass which sperm-whales partake with the whales proper, is of an incontestible inferiority, as it is unfit for graceful movements; but, on the other hand, the

third family, that of *Delphinidæ*, seems to complete the progressive series in the development of teeth; for the latter exist here on both jaws, whence the name of *ambidentated cetaceans*. The fourth family, that of *Heterodontidæ*, includes the narwhale or *predentated cetaceans*, and some other types in which the dentition is losing both its shape and its function. The *Monodon* (narwhale) is closely allied to *Phocæna* (porpoise), whilst *Hyperoodon* comes nearer to *Delphinus*. The other genera are deviations or reminiscences of the other families. Heterodonts, then, must follow the dolphins in a natural and serial classification. The order of Cetacea begins with the whales, and closes with heterodonts; the real superior groups are those placed in the middle, the Delphinidæ, which represent the normal cetacean type. They are the smallest of the order, and possess two fresh-water representatives, one closely allied to dolphins proper, the second bearing some far relations to Physeteridæ (sperm-whale), and to the genus Hyperoodon of the heterodonts family.

The morphology of the teeth in Cetacea is very interesting, and instructive in a philosophic point of view, when the relationships of this order with the Edentata are well understood. In the lowest type, teeth remain undeveloped; in the highest, they cover the whole surface of both jaws, but are of one kind: incisors, canines and grinding teeth are not known amongst cetaceans. This fact alone would ascribe to them an inferior rank amongst the normal groups of the class.

§ 4. The affinities of the so-called herbivorous cetaceans, or Sirenidæ, with pachyderms, have been alluded to by several authors. In 1834 FRED. CUVIER* wrote the following remarkable sentence: "The group of herbivorous cetaceans, composed of genera intimately connected together, are related to the pachyderms by the "manati." And farther on (page 6) he remarks that they come nearer to pachyderms than to cetaceans. In 1838 they were definitively removed from the Cetacea, and actually placed amongst Pachydermata†. Upon this point, every naturalist now agrees. Sirenidæ are the lowest grade among pachyderms: even if considered as parallel

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material strength is developed, and the muscular power increased to harmonize with the immensity of the element in which they live. Balænidæ, the lowest of the order, are likewise amongst the largest.

* Histoire Naturelle des Cétacés, p. 34.

† OWEN, in Proceed. Zool. Soc., London.

to pachyderms, they still must rank lower in a natural classification. They are aquatic, provided only with the anterior limbs constructed for swimming. Unlike the cetacea, they live near the land, and may occasionally creep along a beach; undoubtedly representing a higher step in the class, and an approximation towards the subaquatic Hippopotamus, which, together with the tapir, show intimate relation with the manati and dugong. The Dinotherium, and other fossil representatives of the group of Sirenidia, seem to synthetise the living genera of their groups together with both the proboscidian pachyderms and the ruminants. This synthesis, however, cannot yet be fully understood. The earth's crust has not yet yielded all the data by which alone we delineate the history of the pachyderms and allied groups from their cradle up to our days.

Amongst the living genera, we observe the following particulars: The *Manati*, when young, have on the lower jaw two small incisors directed forwards and downwards, reminding us of the tusks in Dinotherium. The presence of tusks, therefore, assigns to the latter a lower position. In *Halicore*, tusks exist on the upper jaw, as in the elephant, with which the genus *Rytina* seems also related by its teeth, although completely deprived of tusk of any kind.

§ 5. The position of the *Walrus* is between Sirenidia and Pachydermata; they belong to the pachydermic order by structural evidences, and bear only analogies to the seals. They constitute a small group whose distinctive features from Manati consist in the presence of four locomotive members; and from the other pachyderms, in having these four locomotive members adapted for aquatic habits.

§ 6. The order of *Pachydermata* is the least understood of all, on the very ground that its history belongs chiefly to the past; and since Sirenidia and Trichechidæ (walrus) are referred to the same group, it becomes difficult to determine the relationships between the living and the extinct representatives in order to establish a graduated series.

We are satisfied of the existence of two progressive series in the pachydermic groups, in the following way:

WITHOUT PROBOSCIS.	PROBOSCIDIANS.
EQUIDÆ,	ELEPHANTIDÆ,
SUIDÆ,	MASTODONTIDÆ,
HYRACIDÆ,	RYTINIDÆ,
RHINOCEROTIDÆ,	HALICHORIDÆ,
HIPPOPOTAMIDÆ,	MANATIDÆ,
TRICHECHIDÆ,	DINOTHERIDÆ,

ANOPLOTHERIDÆ,
PALÆOTHERIDÆ.

At the bottom of the order, the extinct Palæotherium and Anoplotherium : on one side the proboscidians, and on the other the families which have no proboscis. The proboscidians are relatively inferior to nonproboscidians, inasmuch as they are *edentata* in the general sense of the word : grinding teeth and tusks alone exist. In the nonproboscidians the dental system acquires a great development, the greatest to be observed in the edentated trunk ; but as this development is an excessive effort, and thus brought the group beyond its circle of activity, it had only a temporary existence, and became almost extinct in the present era.

The history of pachyderms will form a contrasting episode compared to that of Cetacea, when it shall once be written out fully. Our hypothetical views on the subject, for fear that they should appear too premature, we abstain from giving now.

§ 7. As to the limits of the order of *Ruminantia*, every one is agreed ; but not so with regard to its systematic position. Considering its imperfect dental system, we see that it belongs to the great division of edentated mammals. That ruminants are inferior in rank to rodents, we derive first from their appertaining to the edentated division, which we have seen is inferior to the division of marsupials. Their dentition and herbaceous diet is a second very important feature which assigns to them a lower rank than to the rodents, which feed chiefly on bark and fruits, a food superior to grass and leaves.

§ 8. Now the position of the order *Rodentia* is clearly defined by what has just been said of the ruminants. Their complete system of dentition, and the similarity in the insertion of the incisors in herbivorous marsupials, are the reasons which have guided us in this arrangement.

§ 9. The place which we assign to the order of *Insectivora* is based upon a similar principle : the affinity of their dentition and mode of life with the insectivorous marsupials and edentata.

§ 10. *Pinnipedia* have always been placed below Carnivora, and Carnivora have always been divided into *digitigrada* and *plantigrada.* We find both plantigrada and digitigrada synthetically indicated in Pinnipedia; not in the structure of the locomotive members, but in the profile of the face.

§ 11. In the eccentric groups of *Bradipodidæ, Sciuridæ, Cheiroptera* and *Quadrumana,* we observe the remarkable fact that they assume a general external resemblance to each other, that they become monkey-like in features and habits. They live above the ground, in trees and in the air; they are chiefly nocturnal, and their diet has a general tendency to becoming frugivorous. That Cheiroptera proceed from the insectivorous stem, the Quadrumana from the carnivorous stem, the Bradipodidæ from the tardigrade stem, a thorough comparison of these types will convince every one.

We give now the following Mammalian System:

I. QUADRUMANA.
- SIMIADÆ.
- CEBIDÆ.
- LEMURIDÆ.
- GALEOPITHECIDÆ.
- CHIROMYIDÆ.

II. CARNIVORA.
- *a.* UNGUICULATA.
  - 1. DIGITIGRADA.
    - FELIDÆ.
    - HYÆNIDÆ.
    - CANIDÆ.
    - VIVERRIDÆ.
    - MUSTELLIDÆ.
  - 2. PLANTIGRADA.
    - CERCOLEPTIDÆ.
    - PROCYONIDÆ.
    - URSIDÆ.
- *b.* PINNIPEDIA.
  - PHOCIDÆ.

III. CHEIROPTERA.
- *a.* FRUGIVORA.
  - PTEROPODIDÆ.
- *b.* CARNIVORA.
  - VESPERTILIONIDÆ.
  - VAMPYRIDÆ.

IV. INSECTIVORA.
ERINACEIDÆ.
SORICIDÆ.
TALPIDÆ.

V. HERBIVORA.
a. RODENTIA.
SCIURIDÆ.
CASTORIDÆ.
MURIDÆ.
Myoxina.
Dipodina.
Ctenodactylina.
Murina.
Spalacina.
Arvicolina.
Bathyergina.
Saccomyina.
HYSTRICIDÆ.
Hystricina.
Dasyproctina.
Echymyina.
Octodontina.
Chinchillina.
Caviina.
LEPORIDÆ.
b. RUMINANTIA.
CAMELEOPARDALIDÆ.
CAMELIDÆ.
ANTELOPIDÆ.
CERVIDÆ.
MOSCHIDÆ.
BOVIDÆ.
c. PACHYDERMATA.
EQUIDÆ.
SUIDÆ.
HYRACIDÆ.
RHINOCEROTIDÆ.
HIPPOPOTAMIDÆ.
TRICHECHIDÆ.
ELEPHANTIDÆ.
MASTODONTIDÆ.
RYTINIDÆ.
HALICHORIDÆ.
MANATIDÆ.
DINOTHERIDÆ.
ANOPLOTHERIDÆ.
PALÆOTHERIDÆ.

VI. CETACEA.
HETERODONTIDÆ.
DELPHINIDÆ.
PHYSETERIDÆ.
BALÆNIDÆ.

VII. MARSUPIALIA.

a. CARNIVORA.

THYLACINIDÆ.

DIDELPHIDÆ.

DASYURIDÆ.

b. INSECTIVORA.

PERAMELIDÆ.

c. HERBIVORA.

PHALANGISTIDÆ.

PHASCOLOMYIDÆ.

MACROPODIDÆ (*Halmaturidæ*).

VIII. EDENTATA.

a. TARDIGRADA.

BRADIPODIDÆ.

MEGATHERIDÆ.

b. EDENTATA PROPER.

DASYPODIDÆ.

ORYCTEROPODIDÆ.

MYRMECOPHAGIDÆ.

MANIDÆ.

c. MONOTREMATA.

ECHIDNIDÆ.

ORNITHORHYNCHIDÆ.

IV.

§ 1. The data relating to the earliest appearance of the class of Mammalia lead us as far back in the earth's history as the period of the oolite. There we find it displaying but a small number of forms under the shape of marsupials, more intimately allied, however, to our opossum tham to any of the Australian types. These first representatives of the class inhabited that geographical portion of the globe now called the British Islands.

The conclusions to which CUVIER had arrived, viz. that the epoch of the appearance of mammals was the tertiary in the series; his beautiful researches, his remarkable discourses on the revolutions of the globe, were present to the mind of every one. Now came that fossil jaw of an opossum-like animal, which seemed to contradict these philosophical deductions. The mammalian nature of the jaw was denied by some, exaggerated by others: its geological position in the oolite was considered as accidental; but all attempts at rejecting these remains from the class of mammalia have proved unsuccessful; time and repeated investigations have concurred in showing that they were true mammals, and that they truly belonged

to the oolitic period. And instead of contradicting the formerly ascertained results, these facts now complete the palæo-history of the class, and illustrate most beautifully the gradual introduction of the different groups of the animal kingdom upon the surface of our globe. For it remains true that the class of mammalia acquired a full development during the tertiary epoch only; the tertiary types were preceded in the secondary epoch by these marsupials, and in some sort foreshadowed, predicted by them. The marsupials being zoologically inferior, they are geologically the first created. Their abnormal forms, the disproportions of some of their limbs, illustrate the first evolution of the mammalian activity. Their bringing forth their young in an imperfect state of development, and the existence of an external pouch to protect that progeny, assign to them an inferior rank. The fact that there are among them carnivorous, insectivorous, and herbivorous types, indicates clearly that they combine these groups of which they are the prototype in the Creator's thought, and their precursors in time.

As the development of the class went on, and the foreshadowed groups appeared as distinct and independent manifestations of the mammalian organization, the marsupialian group was preserved within the limits of its original conception up to our epoch, in which it stands as an odd group which reminds us of a past order of things. In the actual fauna, Marsupialia are an isolated type which has deceived and misled all the systematic writers; still combining characters of several other types, if it is not understood that they are prototypic, and the lowest, they will give rise to contests as to their position in the system.

No facts illustrate better the immateriality of the relations which exist between the various groups of this class: they may foreshadow, they may prophetise, but they will continue to exist. There are no material transformations, no material permutations, from one group to another; for if such was the case, those first created groups, combining those of a later appearance, would not be found possessing the same material attributes, the same circle of vital activity as before. On the other hand, when the foreshadowed groups appear, they lose their zoological importance, and accordingly are confined to a geographical province physically lower, to remind us of their low position in the system.

§ 2. But if Edentata are zoologically the lowest of the class, they should have been created the first in time, or at least be contempo-

raneous with Marsupialia. As far as our present knowledge respecting the fossil remains of mammals goes, Edentata are not known prior to the miocene strata of the tertiary epoch; but in those very strata, their remains are so numerous, and exhibit such a diversity of generic forms, that we must conclude from these facts that Edentata have acquired, if not the maximum of their development, at least a large portion of it, during the first period of their creation.

This great development of Edentata, at the presumed dawn of their existence, is in contradiction with the general law which has presided over the development of all other groups of the animal kingdom: each group, each natural order or family, the history of which has been investigated in past times, has manifestly shown a development parallel to that of the individual life: 1st, an early period—corresponding to that of youth—during which the group has but a small number of representatives; 2d, a period of full development—corresponding to that of the adult—during which the group exhibits the greatest diversity which was in its power to assume; 3d, finally there is a period of decline—corresponding to old age and fall—during which period the individuals are less numerous. In the class of Mammalia there are comparatively few groups which have thus reached the third period of their history, and passed away from the surface of our earth. The majority have just attained their period of fullest development at the beginning of the human era, and are actually in existence upon the external surrounding crust of our planet.

According to these facts, and satisfied that the systematic position which we have assigned to Edentata is natural, and in accordance with the general plan of the creation, we predict that remains of Edentata will be found in the strata below the miocene; that they will be found in secondary beds at least as low as the Oolite, if not farther down. If they prove to be of a decidedly lower zoological grade than Marsupialia, they must have been introduced on earth before the latter; and if parallel with them, they must have been contemporaneous. In the actual era, the order of Edentata is in its period of decline: its representatives now living are much less numerous than the extinct ones already known.

§ 3. The Pachydermata constitute another group, whose history chiefly belongs to past times. They are known to have existed as early as the eocene period; the miocene is the period of their greatest dvelopment; they diminish in number in the pliocene, and

finally the living representatives are still less numerous. So that pachydermata are in the period of their decline, as well as edentata.

Now as far as is known, these two groups, Pachydermata and Edentata, are the only ones in the class of Mammalia whose circle of activity has been exhausted in geological ages.

The two series which we have established among pachyderms will have to be carefully studied geologically.

The oldest remains known of Sirenidia have been discovered in the lowest beds of the miocene period.

The oldest remains of ruminants known, belong to the middle strata of the miocene period.

Cetaceans are contemporaneous with the ruminants; it being always understood that we speak of the actual state of our knowledge.

Rodentia, which we consider the highest amongst Herbivora, are foreshadowed by Marsupialia, the second synthetical type. Rodentia make their first appearance at the beginning of the eocene period, the first of the tertiary epoch.

And so do the Carnivora proper and Pinnipedia, parallel in their genetical development; although, zoologically speaking, Pinnipedia are lower, and synthetise the two groups of carnivorous digitigrades and plantigrades.

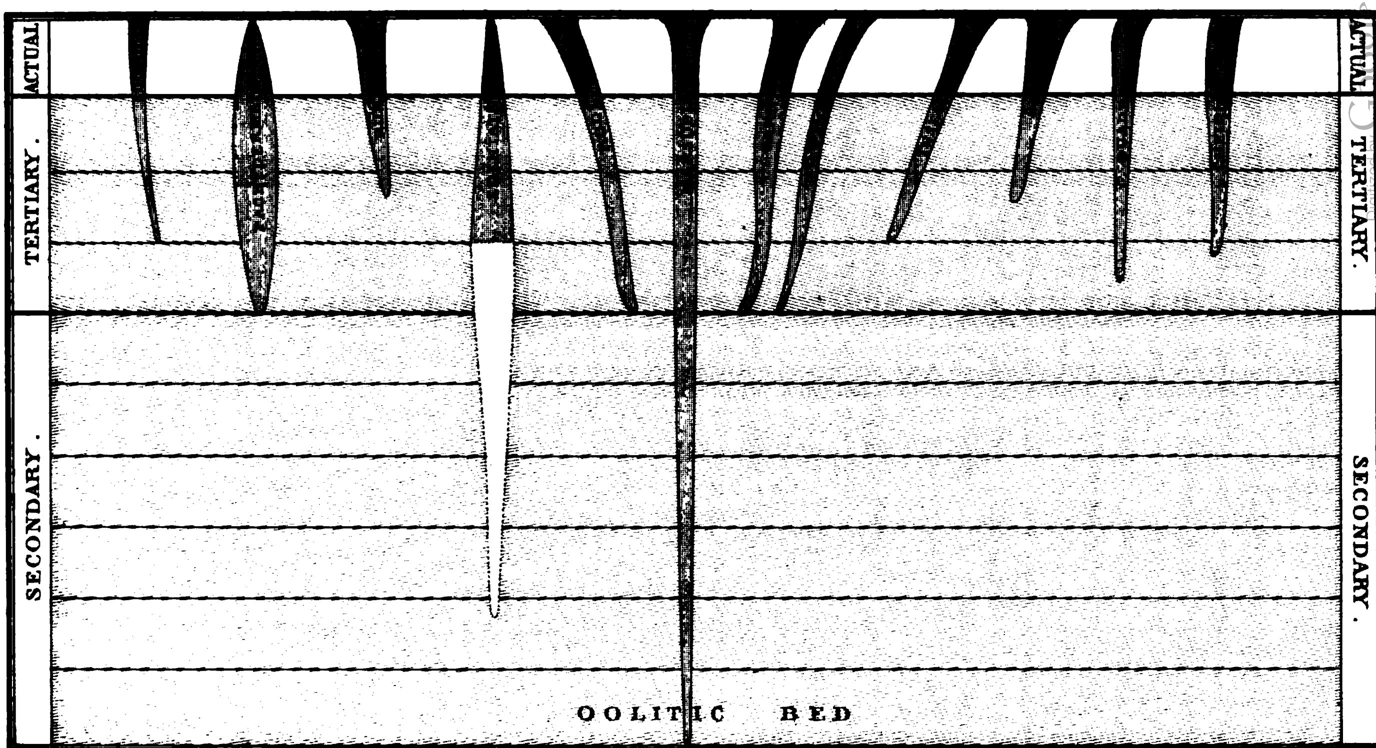
Insectivora, which are shadowed by both Edentata and Marsupialia, are not known in the eocene: their remains, hitherto found, belong to the miocene and strata above.

Quadrumana and Cheiroptera have left some of their remains in the middle strata of the eocene period.

The annexed diagram is intended to sketch out the history of the class of Mammalia, prior to the epoch of mankind.

§ 4. If we look now at the geographical distribution of Mammalia, which is regulated by laws, we may point out some facts of a very striking interest, and which corroborate the foundation of our classification.

The globe and the animal kingdom were created for one another; the globe, however, was made for the kingdom, matter being subordinate to life. During each of the geological ages, and even during each period or era, the physical features of the globe have assumed a peculiar character. The animal creation has likewise assumed a peculiar zoological character, always in a direct relation with the



CHRONOLOGICAL CHART OF THE CLASS OF MAMMALIA.

physical characters of the time and the special physical wants of the globe.

There are two points of view to be taken into consideration when investigating the introduction of life upon the surface of our globe, but these we cannot discuss at length here : we must limit ourselves merely to the signalizing of them.

1. Life, from its first manifestation upon the globe, may have undergone a gradual, slow, and continuous development; in which case a single and unique creation, passing through divers metamorphoses to suit the wants of the globe, renewing itself without the necessity of a special creation at the beginning of each period, would seem the real doctrine.

2. Life, after its first introduction on earth, might have ceased at the end of each period, and, at the beginning of each one, a new creation called forth, purposely made to suit the physical wants of the new era. Thus numerous creations would have succeeded each other without any material connection, or any genetic relationship, but physically independent of each other.

Both of these views have their defenders and opponents. The choice of one or the other is of no consequence in regard to the fact which we are now tracing, as soon as we can admit that *at each period the animal kingdom was in a direct relation with the physical wants of the globe.*

The physical wants of our planet went on increasing with Time, both in number and importance; and the same may be said of animal life. The relations of these two worlds are so intimate, that the zoological subordination of the groups will give us the relative physical superiority of the continents above one another; and, *vice versa,* the relative physical superiority of the continents will point to the zoological gradations of the groups composing the class of mammalia.

Now let us look at the facts. The lowest Mammalia, the Monotremata, belong exclusively to Australia. Australia is physically the lowest continent. Marsupialia are also limited to the same continent.

The next in grade after Monotremata are the Edentata proper, which belong chiefly to South America; the Manidæ and Orycteropodidæ alone being African. South America and Africa rank above Australia; and although Marsupialia are placed by us above Edentata generally, the consequence of their occurring in Australia does not contradict the assumption that Australia is physically lower than

Africa and South America. The fact that the lowest among Edentata are Australian, and the highest among Marsupialia (the Didelphidæ) are South American, is very conclusive.

The occurrence of the opossum in the southern part of the United States clearly indicates that this continent is physically inferior to Europe and Asia.

When comparing the relative superiority of the continents with each other, the comparison, in order to remain true, must be made independently of the influences of man. They must be taken at the dawn of their history, when in formation, during the epochs which have preceded the cradle of mankind. If America occupies a relatively low physical rank, that nation by which it has been taken possession of, by which it has been subdued and conquered, has changed its destinies by applying to its elevation the power of its intellectual aptitudes.

Although some few fossil remains of Marsupialia and Edentata occur out of the actual geographical provinces of these groups, the greatest number are found within the limits of the said provinces; showing that the order which now prevails at the surface of our globe, takes its roots in former ages; that the same general laws which now prevail, have presided over the past.

Amongst the normal groups of the class we have Cetaceans, the lowest, all aquatic; as are likewise Sirenidia, Trichechidæ, and Pinnipedia. The pachyderms are tropical : their actual distribution on earth is to be referred to a past order of things, in order to be understood. The Ruminants, Rodents, Insectivora and Carnivora are distributed all over the globe in given proportions.

A general glance at the mammalian fauna of North America strikes us by the preponderance in the number of species of the order Rodentia. The true grass-feeders, the Ruminantia and Pachydermata are in minority; although the new world has been opposed to the old, and called the *continent of vegetation*, by contrast with that of *animalization*. The greatest Carnivora are absent from America : Carnivora are the most numerous where ruminants are most numerous, the former feeding chiefly upon the latter.

Each group has a part to perform in the economy of nature. Carnivora, the most powerful in the animal creation, check the ruminants, the most bulky and most clumsy of the terrestrial forms of the class, and partly the rodents; the rodents, in their turn, check the arborescent vegetation, whilst ruminants check chiefly the grass.

Ruminants are constructed to walk on the surface of the ground; whilst the organization of rodents is adapted either for ascending trees, or for burrowing in the ground. Ruminants are timid, constantly in fear of becoming the prey of others, and have for their only retreat the depths of the forests, or the unbounded plains and deserts.

The Insectivora feed upon Articulata, and are intended chiefly to check the never-resting class of Insects : they are adapted to divers situations ; for the aerial element, the surface of the soil, and under it, as their peculiar instinct will lead them to feed either on flying, creeping, or burrowing articulates. The Insectivora increase in number from the north to the equator, as the class of insects does.

Amongst the eccentrical types, the majority of the species inhabit the warm zone ; a very significant fact. Cheiroptera exist in both hemispheres, increasing in number from the arctic regions to the tropics. Quadrumana are chiefly tropical ; and so are Bradipodidæ. Flying squirrels belong to the temperate and tropical zones.

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9. On the Preservation of Animal Substances. By Dr. H. Goadby.

Dr. Goadby's preserving fluids are made according to the following formulæ :

A 2.

Rock salt	4 ounces.
Alum.............	2 ounces.
Corrosive sublimate,	4 grains.
Water	2 quarts (*imperial* of 40 ounces).

B.

Rock salt	8 ounces.
Corrosive sublimate,	2 grains.
Water	1 quart, imperial.

The A 2 fluid only differs from the A 1, by the employment of *two* quarts of water to the same ingredients, instead of *one quart*.

B fluid, made according to the above recipe, is adapted to the preservation of land and lacustrine animals : marine animals require about two ounces more salt to one quart of water. The sp. gravity should be 1,148.

III. PHYSIOLOGY.

10. Influence of the Poison of the Northern Rattlesnake *(Crotalus durissus)* on Plants. By J. H. Salisbury, M. D., of Albany.

On the eighteenth of June, 1851, a large female rattlesnake, which had been caged in the New-York State Cabinet of Natural History for about a year without food, died. On dissection, its stomach and intestinal canal were found entirely empty, as much so as if they had been scoured out with soap-suds. The sack in which the poison is emptied was laid open, and the virulent matter (of which there was but little) carefully removed and placed in a porcelain capsule. About fifteen minutes after its removal, four young shoots of the lilac *(Syringa vulgaris)*, a small horse-chestnut of one year's growth *(Æsculus hippocastanum)*, a corn plant *(Zea maiz)*, a sunflower plant *(Helianthus annuus)*, and a wild cucumber vine, were severally vaccinated with it. The vaccination was performed by dipping the point of a penknife into the poisonous matter, and then inserting it into the plant, just beneath the inner bark. No visible effect, in either case, of the influence of the poison was perceptible till about sixty hours after it had been inserted. Soon after this the leaves above the wound, in each case, began to wilt. The bark in the vicinity of the incision exhibited scarcely a perceptible change; in fact it would have been difficult to have found the points, if they had not been marked, where the poison was inserted. Ninety-six hours after the operation, nearly all of the leaf-blades, in each of the plants above the wounded part, were wilted and quite dead. On the fifth day, the petioles and the bark above the incisions began to lose their freshness; and on the sixth, they were considerably withered. On the seventh day they appeared about as they did on the sixth. On the tenth, they began to show slight signs of recovery. On the fifteenth, new but sickly appearing leaves began to show themselves on the lilacs, and the other plants began to present slight signs of recovery in the same way. Neither of the plants were entirely destroyed.

It was interesting to mark the progressive influence of the poison. The first indication of the derangement of the healthy functions of

the plants was observed in the leaves : these began to wilt and die at their edges and apices; and this death gradually and uniformly advanced on all sides towards the middle and petiole, till the whole or nearly the entire leaf was destroyed.

It is an interesting fact in physiology, that the plants first exhibited signs of death in the leaves; and still more interesting, that this death commenced first in the leaves on the side of the plant in which the poison was inserted*.

The facts materially deducible from these experiments are:

1. That the effects of the poison of the rattlesnake upon plants and animals, when introduced into their circulation by a wound, are similar†.

2. That it requires a much longer time for it to affect the plant, than the animal‡.

3. That the effects were invariably exhibited on the part above the wound, and in no case affected the leaves below it||.

4. That it invariably affected first the leaves on the side of the plant in which the incision was made.

5. That its influence was invariably first rendered visible on the edges and apices of the leaf-blades§.

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* This shows a less perfect system of anastomosing vessels than exists in the animal.

† It is stated, on good authority, that the poison of the snake can be taken into the stomach of the animal with impunity; its dangerous effects being only exhibited when introduced into the circulation by a wound. It would be interesting to note its influence on the animal when applied externally to the skin, to see whether its deadly effects would be modified by the absorbents.

‡ It should be stated, in order to show that animals were readily affected by the poison of the snake, that a short time previous to its death, a rat bitten by it died in about two hours.

|| This was probably owing to the small quantity of the poison inserted in each case.

§ In this respect the effects of this poison on plants resembles strikingly, in its beginning and progress, the disease which has for the last few years affected so much the potato.

11. OBSERVATIONS ON THE FREEZING OF VEGETABLES, AND ON THE CAUSES WHICH ENABLE SOME PLANTS TO ENDURE THE ACTION OF EXTREME COLD. By JOHN LE CONTE, M.D., Professor of Natural Philosophy and Chemistry in the University of Georgia.

IN the years 1775 and 1777, JOHN HUNTER communicated to the Royal Society two series of experiments on the "Heat of Animals and Vegetables," from which he drew the inference, "that an animal must be *deprived of life* before it can be frozen;" and "that plants when in a state of actual vegetation, or even in such a state as to be capable of vegetating under certain circumstances, must be *deprived of their principle of vegetation* before they can be frozen" (Phil. Trans. for 1775, pp. 452 and 454). Again, he says, "But the question is, is every tree dead that is frozen? I can only say, that in all the experiments I ever made upon trees and shrubs, whether in the growing or active state, or in the passive, that whole or part which was frozen, was dead when thawed" (Phil. Trans. for 1778, p. 40). With respect to *animals*, Hunter concluded from his experiments, that when the *whole* was frozen, the actions of life could *never* be restored; but that the ears of rabbits, and the combs of cocks were frozen *without injury* to the parts (Phil. Trans. for 1778, p. 34 et seq.). More recent observations have convinced animal physiologists, that Hunter's generalization was premature; and that a degree of cold which absolutely freezes their bodies *is not* equally fatal to all classes of animals. The warm-blooded vertebrata are destroyed by it; and many insects, in their perfect state, are said to suffer in like manner. On the other hand, many fishes, and some reptiles, may be completely frozen without their vitality being necessarily lost. Sir John Franklin, Pallas, Bell, and many others relate, that certain species of fish, which are found imbedded in the ice of the polar regions, are restored to life when thawed. Hearne, in his Journey from Hudson's Bay to the Northern Ocean, mentions his having found various species of frogs so completely frozen that their legs were as brittle as pipe-stems, and which resumed their natural movements when exposed to a genial heat. If permitted to freeze again after being thawed, they never recovered. He found spiders and grubs in a like frozen condition, with the same powers of revivification on exposure to a warm atmosphere (Vide Hearne's Journey from Prince Wales' Fort, Hudson's Bay, to the Northern

Ocean, pp. 397 et 398, Lond. 1795). The *larvæ* of insects are equally tenacious of vitality. Lister, Bonnet, and others, have found caterpillars so frozen that, when dropped into a glass, they chinked like stones; but that they nevertheless revived. The *Papilio brassicæ* has been produced from a larva which had been exposed to a cold of 0° Fahr., and which had become a lump of ice. Spallanzani found that exposure to a temperature of —38° or even —56° Fahr. did not destroy the fertility of the ova of silk-worms; and the eggs of the slug have been subjected to a cold of —40° Fahr. without injury. The following experiment upon caterpillars, tried in Sir John Ross' voyage, seems to be perfectly satisfactory on this point. Thirty larvæ of the *Laria rossii* were put in a box, and exposed to the winter temperature for three months; on bringing them into the cabin, every one of them returned to life and walked about. They were again exposed to an atmosphere of —40° Fahr., and instantly became refrozen; after a week, they were brought again into the cabin, and twenty-three returned to life. These were again exposed and refrozen; and, after being solid for another week, *eleven* of them recovered on being brought into the cabin. A fourth time they were frozen, and only *two* survived (Vide Carpenter's Principles of General and Comparative Physiology, 2d ed., Lond. 1841, pp. 158, 174 and 175).*

The foregoing facts appear to indicate, that the power of revivification after the complete congelation of the fluids, is confined to animals in which the function of calorification is imperfectly per-

* The recent observations of Prof. James Paget seem to show, contrary to the opinion of Hunter, that it is *not* by the power of a vital principle that eggs resist the influence of cold. His experiments prove, "that certain things will destroy the power of resisting cold, without affecting the capability of being developed, and of therein manifesting the best evidence of life; and that when eggs yield to the influence of intense cold, they are not damaged unless they are frozen, and are *not killed* even when frozen." "The experiments thus remove almost the only remaining support of the hypothesis that such a vital principle may exist in organized bodies, as may enable them, even while inactive and dis-displaying no other signs of life, to resist passively the influence of physical forces." Prof. Paget thinks, that the fact that the temperature of eggs may be reduced far below the freezing point without being frozen, is due, not to any vital principle, but to some peculiarity of the mechanical constitution of the albumen, "by means of which the water combined with it is held so steadily, that the agitation favourable, or even necessary, to the freezing at or near 32°, cannot take place" (Vide Phil. Trans. for 1850, p. 221 et seq.).

formed, and in which all the vital processes are obscurely manifested. As all the functions of vitality are still more obscurely performed in plants, we should naturally expect them to be endowed with a similar power of resisting the destructive effects of freezing. Nevertheless, the most eminent writers on vegetable physiology seem to be very general and decided in the opinion, that the *complete* solidification of the fluids of a plant *necessarily and inevitably results in its death.* For, although it is well known to practical horticulturists, that the pernicious consequences of severe cold on growing vegetables may be, to some extent, obviated by careful and gradual thawing; yet, it is thought that, in such cases, the freezing is incomplete, and does not involve *all* of the structures of the plant. Thus, for example, M. Aug.-Pyr. De Candolle, after enumerating the effects produced by a partial freezing of the internal parts of trees, in which the *alburnum* is the only part attacked and disorganized, giving rise to what are called *gelivures*, remarks : "Enfin, si le gel est assez fort pour attendre le liber, *alors la branche ou l'arbre dont le liber gèle périt presque toujours*, soit que la gelée du liber soit un signe de la gelée totale de l'aubier, soit que le rôle du liber soit lui-même plus important et lié avec la congélation de tous les bourgeons" (Physiologie Végétale, tome 3, p. 1119, Paris 1832). Again he says, "Si elle (la température) descend au-dessous de la congélation, elle solidifie d'abord l'eau située à l'exterieur du végétal, et arrête d'autant la nutrition; puis elle atteint les liquides aqueux renfermés dans le tissu végétal : en les congelant, elle les dilate; de cette dilatation résulte *la mort du végétal ou du fragment de la plante où elle a lieu*, soit, comme l'ont cru plusieurs auteurs, par la rupture des cellules et des vaisseaux (fait que les recherches récentes de M. Gœppert rendent au moins très-douteux), soit par la dénaturation des sucs eux-mêmes que la gelée tend à séparer en parties plus ou moins susceptibles de congélation, soit simplement par un effet vital sur le tissu des cellules" (Op. cit. supra, tome 3, p. 1101). Prof. J. S. Henslow remarks, that, "When the sap is frozen, the cells and vessels in which it is contained are ruptured, *and the parts subjected to such an accident die*" (Principles of Descriptive and Physiological Botany, Lardner's Cabinet Cyclopædia, p. 297, London 1844). Again, the same botanist says, that, "Whenever the sap does freeze, it produces the effect technically termed 'shakes'" (probably the *roulure* of the French) "in timber trees, which consists in a tendency in the separate layers of wood to disunite" (Op. cit. supra,

p. 173). References might be multiplied to show how universal this opinion is among the best vegetable physiologists. Although most of them reject the idea of Hunter, that a plant must be deprived of the principle of vegetation *before* it can be frozen; yet they appear to be almost unanimous in the opinion, that *after* complete congelation it necessarily dies. It is true, that several facts which seem to contravene this opinion have not failed to arrest the attention of several botanists. Thus, M. De Candolle cites the fact, attested by M. Thouin, that the cases of apple-trees sent to Moscow arrived there in a frozen condition, and that a great part of them were saved by gradual and slow thawing (Physiologie Végétale, tome 3, p. 1122). It appears, however, that such phenomena have attracted but little attention, and provoked no scientific research; they have remained barren and isolated facts in the field of science.

From a careful examination of Hunter's experiments, I am surprised that either he or his successors should have drawn such conclusions as have been deduced from them in relation to the effect of cold on vegetables. For example, we will cite his second experiment in his earlier paper: "A young Scotch fir, which had two complete shoots and a third growing, and which consequently was in its third year, was put into the cold mixture which was between 15° and 17° Fahr. The last shoot was froze with great difficulty; which appeared to be owing in some measure to the repulsion between the plant and the water. When thawed, the young shoot was found flaccid. *It was planted:* the first and second we found *retained life;* while the third, or growing shoot, withered" (Phil. Trans. for 1775, p. 451). Again, in his second series of experiments, when the temperature of the air was 16° Fahr., he found a thermometer inserted into the trunks of a number of species of trees to stand at 17° Fahr. Now, he found that the sap taken from the walnut tree on which he made the experiment, would freeze at 32°; and furthermore, that the sap which filled an old hole which he had made in the same tree, became frozen when the temperature of it was 31° Fahr. Assuming that the juices of the tree *were not frozen* when their temperature was 17° Fahr., Hunter very naturally inquires: "Now, since the sap of a tree, when taken out, freezes at 32° Fahr.; also, since the sap of the tree, when taken out of its proper canal, freezes when the heat of the tree is at 31°; and since the heat of the tree can be as low as 17° Fahr. without freezing, by what power are the juices of the tree, when in their proper canals, kept fluid in

such cold? Is it the principle of vegetation?" (Phil. Trans. for 1778, pp. 47, 48). Hunter has not informed us in what manner he ascertained that the juices of the tree were not frozen when their temperature was 17° Fahr.; but the presumption is, that he had no other reason for thinking so, than the fact that the tree *was not killed*. He appears to have been so much prepossessed with the idea that plants "must be deprived of their principle of vegetation before they *can* be frozen," that he never thought of determining, by direct observation, whether or not the sap of the tree was actually frozen when its temperature was 17° Fahr. The very first principles of philosophizing demand, that it should be clearly and undeniably established as a *matter of fact*, that *every case* in which the sap of a plant *is frozen*, is invariably followed by the death of the whole, or, at least, of the part congealed, before the fact of its having survived can be made the basis of the conclusion that congelation of the juices had not taken place. The assumption is made that a plant which is not killed by cold, never was frozen; and then, theories are framed to account for the presumed fact!

Impressed with this fundamental idea, all attempts which have been made by modern phytologists to explain how vegetables endure the action of excessive cold, resolve themselves into an enumeration of the possible causes which may prevent their juices from freezing. M. Aug.-Pyr. De Candolle has investigated the action of these causes with his characteristic sagacity (Physiologie Végétale, tome 3, p. 1101 et seq.). The causes to which this assumed resistance to freezing has been ascribed may be reduced to *five*: 1. A certain amount of *proper heat*, generated by physiological actions; 2. The viscosity of the juices, lowering the freezing point; 3. The distribution of the sap through minute vesicles and capillary vessels, depressing the point of congelation still further; 4. The warmth of the ground from which the sap is pumped up; 5. The low conducting power of concentric layers of bark with entangled air included in their meshes, and of the wood itself, which is less transversely than longitudinally. We shall examine the adequacy of each of these causes to account for the facts.

1. The experiments of J. Hunter, Schœpff, Bierkander, Pictet and Maurice, Schubler and Neuffer, Hermstædt, Nau, Gœppert, and others, have shown that the interior of the trunks of large trees possesses, during winter, a temperature several degrees higher than that of the surrounding air. But, M. De Candolle and others have

very reasonably doubted whether known physical causes might not be sufficient to account for the fact, without the necessity of ascribing it to the result of any physiological action. By recent experiments, however, made with instruments of great susceptibility to changes of temperature — such as the thermo-multiplier of Nobili — MM. Dutrochet, Becquerel and Breschet, have demonstrated, that in those parts of plants in which the vital processes are taking place with activity, an appreciable amount of caloric is constantly evolved. The amount of this evolution of heat is generally *very low*, not more, in fact, than a *single degree* (Fahr.) in the herbaceous parts of actively-growing plants; and as it does not more than counterbalance the effect of evaporation, which is continually taking place from the surface, there is, under ordinary circumstances, so far as *this cause* is concerned, no sensible difference between the temperature of the plant and that of the surrounding atmosphere (Vide *Ann. des Sci. N. S., tome* 12; also, Carpenter's General and Comparative Physiology, 2d ed. Lond. 1841, p. 417). During the winter, when these functions are comparatively dormant, we cannot suppose that they operate at all in resisting any atmospheric changes which might be injurious to vegetation. Nevertheless, as vital changes are taking place with more or less activity at all periods, this may be regarded as a *vera causa;* but its effect, so far as the prevention of freezing is concerned, must be considered absolutely *infinitesimal.*

2. That the freezing point of the juices of plants is but *slightly* depressed below that of water, by the admixture of gum, mucilage, and other products of vegetation which impart viscidity to them, has been demonstrated by direct experiment. Hunter found the freezing point of vegetable juices when squeezed out of a green plant, to vary from 29° to 32° Fahr. In several experiments on the freshly expressed juice of the strawberry, I found the freezing point to vary from 28° to 30° Fahr.; and, in every case, the temperature *after congelation* was 30° Fahr. It is extremely probable, nay, almost certain, that the freezing point may vary with the degree of inspissation of the sap, and may consequently be different for different plants, and at different seasons of the year in the same plant; and, moreover, that it may, on this account, be somewhat lower in winter than in spring or summer. It is also probable, that the admixture of certain peculiar organic products may lower the freezing point of the sap of particular plants. Thus, it is well known to chemists that

the point of congelation of good oil of turpentine is as low as 14° Fahr.; and, perhaps, the presence of this proximate principle may tend to prevent the sap of the coniferæ from freezing in moderate degrees of cold. In the present state of our knowledge, it is impossible for us to assign a definite *quantitative* value to the influence of *viscosity* in lowering the freezing point of the juices of vegetables; but it is certain that it cannot amount to *many* degrees of temperature*. It must have *some* influence, and must thorefore be looked upon as a *vera causa* acting in the *right direction;* but in cases of *extreme cold*, this cause is obviously *inadequate* to prevent the supervention of congelation.

3. Prof. Henslow seems to think, that "the chief protection against the sap freezing in the trunks of trees is the circumstance of its being contained in extremely minute vesicles and capillary vessels; for it has been shown that water will resist a temperature of 16½° Fahr. under similar circumstances; and all viscid fluids are still more difficult to freeze than water" (*Op. cit. ante*, p. 173). It is unquestionably true, that, by taking certain precautions, water may be cooled 15 or even 20 degrees of Fahrenheit's scale below the proper freezing point, without the supervention of solidification. The *essential condition* of success in the experiment, is, that it must be cooled without the *slightest agitation*, and no angular body be in contact with it; for the instant any solid body is dropped into water cooled below its freezing point, or a *tremor* is communicated to it, congelation commences, and the temperature starts up to 32° Fahr. (Graham's Elements of Chemistry, Am. Ed. Phila. 1843, pp. 50 et 51). It is very obvious that this *necessary condition* is most effectually secured by placing the water in *capillary tubes;* for the adhesion of the fluid to the sides of the tubes must tend to maintain it in that state of absolute repose upon which the success of the experiment depends. Thus, Dr. Thomas Thomson succeeded in cooling water in thermo-

* The fact that the *specific gravity* of the sap of plants is but slightly above that of water, affords presumptive evidence that the freezing point can be but little below 32° Fahr.; for experiments on the admixture of salts with water show that the depression of the freezing point is nearly proportional to the increase of *density*. Knight found that the sap of the *Acer platanoides*, in the spring, collected close to the ground, had a specific gravity of 1,004; at 7 feet above the ground it became 1,008; and at 12 feet, 1,012 (Vide Phil. Trans. for 1805, pp. 90 et 91).

meter tubes to 8°, and once to 5° Fahr., before it began to freeze (Vide Heat and Electricity, London 1830, p. 175). In the case of trees and shrubs, we have no means of ascertaining how far the *indispensable condition of absolute repose* may be subverted, by the perpetual agitation to which their branches and more flexible parts are subjected through the action of winds; but it seems to me, that in the case of plants, the distribution of the fluids through capillary vessels can have but slight, if any, influence in the prevention of their congelation.

4. We have already alluded to the fact, established by the experiments and observations of many distinguished physiologists, that the interior of the trunks of large trees possesses, during winter, a temperature considerably above that of the surrounding atmosphere. It has, likewise been shown that this heat *is not* produced by the physiological actions which are taking place in the plant; the amount generated by this cause being wholly inappreciable. M. Aug.-Pyr. De Candolle very justly ascribes this uniformity of temperature of the interior of trees to the circumstance of their roots penetrating the earth to a depth where the soil is always warmer than the atmosphere in winter and cooler in summer, and imbibing moisture which must necessarily partake of this influence. Hence it has been observed, that the internal parts of large trees retain a temperature which is about equal to that of the soil at the mean depth to which their roots penetrate (*Physiologie Végétale*, tome 2, p. 879 et seq.; also tome 3, pp. 1101, 1102, 1108). There can be no doubt that this is the chief cause of the uniformity of temperature of the interior of the *trunks of large trees;* but when considered as a resource against the effects of extreme cold, it is necessary to suppose that its protective action extends to the remotest branch and most minute twig. Now, it will hardly be contended by any one, that a small branch of a tree, situated 80 or 100 feet from the ground, will have its temperature appreciably modified by the tardy circulation of sap which takes place during the winter. In fact, direct observations show that the temperature of the interior of small trees, shrubs, and twigs, is sensibly the same as that of the surrounding atmosphere; and the difference becomes more apparent the larger the trunk on which the observation is made, and the nearer it is to the ground. It is manifest, therefore, that, so far as the buds and smaller branches are concerned, the cause under consideration can have no *practical* influence in enabling vegetables to

resist the action of excessive cold : its effect must be *infinitesimal.* Moreover, it will be shown, hereafter, that perennial plants, and even large forest trees, endure the intense cold of a Siberian winter, *when their roots are imbedded in a soil which is frozen more than one-half of the year.*

5. After what has been said above, it is unnecessary to dwell on the influence of the bad conducting power of the concentric layers of bark, or on the greater facility with which the wood itself transmits heat longitudinally than transversely, as proved by the experiments of MM. Aug. De La Rive and Alph. De Candolle (*Mém. Soc. de Phys. de Genève,* vol. 4, p. 71; also, *Ann. de Phys.* 40, p. 91). These circumstances only prevent the supply of caloric which is pumped up by the roots from the warm earth, from being carried off; but as we have shown that no appreciable amount of this heat can possibly reach the extreme twigs and buds, it is sufficiently evident that the low conducting power of the woody layers and bark can have *no sensible* influence in resisting any atmospheric changes which might be injurious to these portion of plants*.

It is proper to remark, that until quite recently, I participated in the opinion so generally prevalent among the most eminent phytologists, that the sap of trees and shrubs which are uninjured by extreme cold, is *never* frozen. I, therefore, entered upon the investigation with all my prepossessions in favor of the commonly-received opinion in relation to this subject. Nevertheless, the *glaring inadequacy* of all the causes which have been assigned to explain the presumed fact, induced me, during the winter of 1850–51, to institute a series of observations and experiments, with the view of obtaining clearer ideas. The sequel will show that I was very soon driven to the conclusion that the *fundamental idea* is erroneous, and that plants *do become frozen* without the slightest injury to them.

On the morning of the 18th of November 1850, I found the leaves of the common garden cabbage covered with hoar-frost on both surfaces, and so completely frozen as to be quite rigid and stiff. On more minute examination, it was found that the fluid contained in the petioles, as well as that of the midribs and lateral veins, was completely

* These considerations seem to show, that those phytologists who are disposed to look at this subject in a teleological point of view, have mistaken the true office of the *scales* which envelope the buds of plants : they can afford no protection against the action of cold.

frozen. By making a transverse or longitudinal section of them, the *icicles* could be scraped out with the edge of a knife. No indications of congelation could be detected in the parenchyma and smaller veinlets. At the time these observations were made, I had no thermometer with me; but the weather was quite moderate: the temperature at sunrise could not have been below 28° Fahr. The cabbages were uninjured, although most of them were exposed to the direct action of the sun. Subsequently, the leaves of the *Gardenia florida* were, on several occasions, observed to be frozen during the frosty mornings in December. They were curled backwards, and were so rigid as to break when an attempt was made to bend them. The foliage was not injured by this degree of cold, although it is by no means a hardy plant.

Doubtless such facts are familiar to every practical horticulturist: nevertheless, I was anxious to ascertain whether the freezing ever involved the woody structures; and, if so, what was the effect on the plant. As the winter advanced, other opportunities presented themselves for extending these observations. On the morning of the 30th of January 1851, the temperature being 18° Fahr., I examined the larger stems of a number of roses in my garden, and found the fluids of the bark and liber of all of them *completely frozen, presenting a polished vitreous surface when cut by a sharp knife.* The fluids freely exuded from abraded portions of the bark, as soon as they were thawed by the warmth of the hand, or by removal to a warm room. The branches of the *Pinus tæda*, some of them more than a half inch in diameter, were found to be so brittle as to snap under a very slight degree of flexure. The fluids appeared to be congealed; for no gum exuded from the fractured extremity, until it was brought into a warm atmosphere. These observations were repeated, and extended to other perennial plants, on the morning of the 31st of January 1851, when the temperature of the atmosphere was 13,5° Fahr., *with precisely identical results.* It is almost needless to state, that the roses, pines, and other plants examined, were *uninjured.*

The foregoing observations afforded no very satisfactory information in relation to the condition of the fluids of the proper *woody structure*: they only furnished positive evidence with respect to the state of the juices contained in the succulent layers of the *inner bark.* During their progress, I endeavored to examine some transparent sections of the plants by means of a compound microscope; but it was found to be so difficult and unpleasant to carry on such observa-

tions in the open air, that I determined to make use of *artificial cold* in the prosecution of the investigation. With this view, the following experiments were instituted; the frigorific agent being a mixture of snow and common salt.

For the purpose of testing the effects of cold, I selected the internodal portions of a vigorous freshly-cut elder stalk *(Sambucus canadensis)*, of about *one inch* in diameter : the diameter of the pith was nearly *one half* that of the stalk. The pith of this plant is surrounded by a zone about $\frac{1}{20}$th of an inch in thickness, which is more succulent than the central portions of the pith proper, and from which fluid could be readily pressed by lateral compression *in situ* with the back of a knife. A smaller but sensible quantity of fluid could, likewise, be expressed from any portion of the pith, so as to appear on the cut surface, and which would disappear as soon the compressing force was withdrawn. It was also found that fluid appeared at the transverse cut surface of the liber, when pressed against the subjacent wood at the extremities. It was hoped that the application of the foregoing tests would enable us to ascertain, with considerable certainty, whether the juices of these portions of the plant could be readily frozen, independently of any light which microscopic examination might throw upon the question.

Experiment No. 1. Feb. 2d, 1851, 1 o'clock P. M.; the temperature of the room being 42° Fahr., and that of the freezing mixture varying, during the experment, from 2° to 5° Fahr. With the view of excluding the direct contact of the frigorific mixture, the internode of elder was inserted into a water-tight tinned sheet-iron case or cylinder, of nearly the same diameter as the stalk : the whole was plunged vertically, and in the natural growing position, into the freezing mixture, about $\frac{7}{8}$ths of the internode being below the surface. At the expiration of *two hours*, the stalk was removed from the case for examination. The extremity which was above the cold mixture, was found to be surmounted by a thin transparent coating of ice. The fluids seem to have been forced up along the inner bark and the succulent zone surrounding the pith, by the contraction which took place in the part of the stem below the mixture, before congelation supervened. The *lower* extremity was found to be quite free from external ice, and appeared comparatively dry. The fluids of the bark, as well as those of the succulent zone around the pith, *were obviously congealed.* The frozen condition was rendered evident, by slicing the bark longitudinally with a sharp knife : the ice could

be scraped off with the edge of the instrument. In like manner, small masses of solidified juice could be detached from the succulent zone, as well as the pith.

Under the microscope, when a transverse section was made with a knife which was artificially cooled and placed on a plate of glass which was cooled in the same manner, the process of thawing was quite conspicuous and beautiful, especially in the succulent zone. In the *woody structure*, the process of liquefaction was manifested by the rapid appearance of *bright apertures*, which were merely translucent points when first placed under the glass. Slices of wood which had not been subjected to the influence of cold, presented *no changes* under the magnifier. As it is necessary to make the slices very thin, in order to secure the requisite degree of transparency, the thawing takes place with great rapidity. The observations must, therefore, be made quickly : the knife with which the slices are made, as well as the glass plate on which they are placed for examination, should be artificially cooled.

Experiment No. 2. In the previous experiment, the lower extremity of the internode of elder was exposed to the action of the cold; and it might be supposed that the frigorific influence was propagated along the stalk *longitudinally*, the direction in which the experiments of MM. Aug. de la Rive and Alph. de Candolle show the conducting power of wood to be the greatest. For the purpose of obviating this difficulty, a hollow tin cylinder, six inches in length, open at both extremities, was passed transversely through the centre of the opposite sides of a small wooden box. Another recently-cut internode of elder was thrust through the cylinder, having both extremities projecting several inches beyond the ends of the metallic case, so that only *the middle portions* would be subjected to the frigorific influence. On the sixth of February 1851, at one o'clock P. M., the temperature of the room being 37° Fahr., the freezing mixture was introduced into the box. At the end of *two hours*, the temperature of the mixture varying from 2° to 6° Fahr., the stalk was withdrawn for examination. The bark in the middle of the part which had been exposed to the cold, *was found frozen* as in the previous experiment : neither the bark, succulent zone, pith, or wood at *the extremities*, exhibited any indications of congelation of the fluids contained in them. The middle of the stalk was then cut through by means of a small saw which had been artificially cooled. On examining the transverse section, the liber and succulent zone around

the pith were evidently frozen : portions of solidified sap could be removed with the point of a knife. Under the microscope, the thawing of thin transverse slices of the woody structure was manifested by a series of changes identical with those observed in the first experiment. Indeed, the physical properties of the wood were sufficiently modified to indicate the frozen condition by the use of a cutting instrument : it was harder than natural, and *the section presented a polished glassy appearance.* At the same time, a portion of the branch of a *Pinus tæda* was subjected to the same degree of cold : the microscope revealed the fact that its juices were, likewise, frozen; a similar change, indicative of thawing, being clearly observable.

Experiment No. 3. The foregoing experiments were tried on portions of plants which had been recently cut from the parent stem or branch; and it might be imagined that the case would be different with *a growing* plant. For the purpose of testing the validity of this idea, two vigorous shoots of the *Ailanthus*, which had been planted in a large box of earth, were, on the nineteenth of February 1851, submitted to experiment. The temperature of the room was 56° Fahr. : that of the freezing mixture varied from 0 to 5° Fahr. Tin tubes were placed around the base of the stems, so as to protect them from the immediate contact of the mixture. The frigorific material was kept around each tube by means of two small wooden boxes which were perforated at the centre of the bottom, so as to permit the tubes and contained stems to pass vertically through. At the expiration of *four* hours, the cold mixture was removed from one of the shoots, and it was cut through the middle of the part which had been exposed to the cold. The juices of the bark and wood were evidently frozen. This was rendered obvious by placing transparent slices of them under the microscope, as in the previous experiments. It was likewise proved by the fact, that at first, when cut with a cold knife, none of the juices were forced out by the passage of the cutting instrument; but after a short time, when thawing had taken place, any attempt to slice the wood transversely would render the presence of the fluids manifest on the cut surface. The *other shoot* was allowed to remain undisturbed for *six hours :* after which, the frigorific mixture was removed, and the box of earth in which it was planted placed in the open air, for the purpose of ascertaining what effect the cold might have on its *vitality*. About the middle of March the buds began to swell, and in due season the leaves were developed. The shoots of *Ailanthus* on which these experiments were tried, were suckers of two years growth.

From the preceding experiments, two conclusions seem to be legitimately deducible : *First*, that the sap of certain plants *can be* readily frozen by the application of a comparatively *moderate* degree of cold. For although the temperature of the frigorific mixture varied from 0 to 6° Fahr., yet as there was a metallic tube surrounding the stem, the temperature of the latter must have been considerably higher. Direct observation proved this to be the case : a thermometer, introduced into the metallic case, never indicated a temperature lower than 15° Fahr. It must also be borne in mind, that as the *duration* of the frigorific action was never more than from *two to four hours*, the *central* portions of the stems submitted to experiment must have possessed a temperature notably *higher* than the exterior parts ; and yet the structures around *the pith* were found congealed. Moreover, a degree of cold equal to zero of Fahrenheit is quite *moderate* in comparison with the natural cold endured by the very plants on which the experiments were performed.

And *secondly*, that congelation of the juices of vegetables *does not*, as many physiologists imagine, necessarily and inevitably result in the death of the whole plant, or of *the part* in which it takes place ; but, on the contrary, that frequently *no injurious consequences follow*.

With such a state of facts before us, we should naturally expect, that in high latitudes, the sap of all perennial plants must be frozen during several of the winter months. *Such, I believe, is the fact.* Robert Boyle informs us, on the authority of Capt. James, that at Charleton Island (now called Charles Island) in Hudson's Bay, *the trees had to be thawed by fire before they could be cut down* (Vide Boyle's Works, vol. 2, p 274, London 1744). I have been credibly informed, that the "*lumberers*" of Maine and New-Hampshire are familiar with the fact, that during periods of extreme cold, the sap of many of the forest trees becomes so completely frozen, that the physical qualities of the wood are altered to such an extent, *that it is difficult to cut it with an axe.* M. Duhamel observes, that the maples in Canada, where the frost is long and severe, begin to bleed, when wounded, with the first thaw, and stop again when it freezes ; and that this, in frosty days, occurs only on the south side of the tree (*Physique des Arbres*, vol. 2, p. 258). In all of these cases it is sufficiently obvious that the freezing of the sap could not have been fatal to the life of the trees ; for, in that event, a single severe winter would destroy every plant.

But, perhaps, it may be granted, that the foregoing experiments

and observations are sufficient to prove that the juices contained in the *aërial or ascending stems* of many plants may be frozen without destroying the power of vegetation; and yet, it may be asked, does not the complete congelation of the fluids included in *all parts* of the vegetable, comprehending the root or descending axis and its appendages, *necessarily result* in the destruction of its vitality? Even the admission that *large portions of the aërial stem* may be frozen with impunity, impresses the character of error upon the commonly received opinion among many of the most eminent phytologists; but I think facts are not wanting to show, that the juices contained in *every part of some plants* may be congealed without injury to the powers of vegetation. Although Leopold von Buch and many other philosophers, even as late as 1825, were disposed to reject the statement of the elder Gmelin, that the ground is perpetually frozen in the northern parts of Siberia; yet the fact has been abundantly corroborated in our days by the observations of MM. Adolph Erman, von Humboldt, Hausteen, Schergin and others. At the town of Yakutsk, in latitude 62°, M. Schergin attempted to sink a well, and was about to abandon the project in despair of obtaining water, when Admiral Wrangel persuaded him to continue his operations until he had perforated the whole stratum of ice. This he did, and kept a complete journal of the work. The well was dug to the depth of nearly 400 feet, with the following most remarkable results as to the temperature of the ground:

50	English feet	....	18°,5 Fahr.
77	"	....	19°,6
119	"	....	22°,0
217	"	....	27°,5
305	"	....	28°,6
382	"	....	31°,0

(Vide Erman, Comptes Rend. de l'Acad. des Sciences, 1838, tome 6, p. 501; also, von Baer, in Edin. New Phil. Journ., 1838, vol. 24, p. 435). From which it appears that the soil at this place is frozen to the depth of about 400 feet. Observations kept at Yakutsk for a series of years, show that the *mean temperature* of the year is nearly 14°,5 Fahr. (Vide M. Mahlmann's table of mean temperatures, in Kaemtz's Complete Course of Meteorology, English Translation by C. V. Walker, London 1845, p. 177. Erman puts down the mean temperature of this place at 18°,5 Fahr.). In winter, a degree of cold exceeding —58° Fahr. takes place every year. The mean tem-

perature of two winter months is below —40° Fahr.; so that mercury is solid for *one-sixth* of the year, and has been known to remain frozen during *three consecutive months!* (Vide Adolph Erman's Travels in Siberia, translated from the German by W. D. Cooley, Phil. 1850, vol. 2, p. 278; also, Comptes Rendus (Paris), tome 6, p. 502). A comparatively warm summer is joined to this cold winter; the mean temperature of June, July and August, being 56°,75, 65°,75, and 61°,25 Fahr. respectively, and the maximum heat of the summer day is sometimes as great as 77° Fahr. During the 128 days of the year in which there is no frost at Yakutsk, the strata of eternal ice are never thawed to a greater *depth than three feet;* and yet "vegetable life continues, not merely uninjured, but favored in the highest degree by the equable and very rapid increase of heat" (Erman, *Op. cit. supra*, vol. 2, p. 279). This is not restricted to *annual* plants; for M. Erman informs us, that noble larch *(Pinus larix)* forests flourished on the east side of this town, where their roots rest upon inferior strata of perpetually frozen earth, and where even the superficial stratum in which they are imbedded is frozen for nearly *eight months* of the year! As it is physically impossible that the roots of these trees can penetrate deep enough even to *approach* the stratum of *invariable temperature* in this high latitude, the *whole plant*, including the descending axis and its appendages must, during the winter, be exposed to a degree of cold considerably below the zero of Fahrenheit. Is it possible to imagine that the sap of these trees remains unfrozen in a climate where mercury is solid during *one-sixth* of the year, and where the stratum of perpetual ground-ice extends to the depth of 400 feet? Here we have an *extreme case*, in which it is sufficiently obvious that no known combination of vital and physical causes could prevent the fluids contained in these plants from undergoing congelation every winter: the conclusion seems, therefore, to be unavoidable, that they *are actually frozen* at this period of the year, and consequently that such an accident *is not always* injurious to vegetation*.

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* The lowest temperature that has yet been observed on the earth, is probably that noted by Neveroff at this place (Yakutsk) on the twenty-first of January 1838. "The instruments used in this observation were compared with his own by Middendorff, whose operations were always conducted with extreme exactitude. Neveroff found the temperature, on the day above named, to be —76° Fahr." (Humboldt's Cosmos, in Bohn's Scientific Library, translated from the German

Neither is the example of Yakutsk an isolated one; for a great part of the vast territory of Asiatic Siberia, and a smaller portion of Europe and North America, lying north of the isothermal line of 32° Fahr., support extensive forests of Birch *(Betula alba)*, Norway Spruce *(Pinus abies)*, Larch *(P. larix)*, Cembra Pine *(P. cembra)*, Scotch Fir *(P. sylvestris)*, White Spruce *(P. alba)*, American Silver Fir *(P. balsamea)*, and Black Spruce Fir *(P. nigra)*, where the ground-ice is perpetual (Vide Johnston's Physical Atlas). These facts appear to warrant the conclusion of M. Adolphus Erman, namely, that it is now fully established, "that many arborescent plants require, as the condition of their thriving, only the summer heat and the humidity of the air that suits them; and that they are, therefore, not only quite insensible to the rigor of winter, but, in spreading over the plains and mountains of the earth, *are wholly independent of the temperature of the ground or mean temperature*" (Vide Travels in Siberia, Ed. *cit. supra*, vol. 2, p. 360). The development of leaves and vegetation depends less on the temperature of the soil, than on that of the air in the spring and summer: it only requires that the ground should be so far thawed, that the tree may be able to draw from it sufficient moisture for its growth. This is especially true of several species of the coniferæ; for many of them are enabled to brave the most rigorous winters, and pass through all the phases of flowering and fructification, provided the summer be hot enough and of sufficient duration.

But it may be objected, that when the sap contained in the trunks of trees becomes frozen, it *cleaves* them with a great noise — a phenomenon not uncommon in high latitudes; and that, therefore, the fact that the majority of them *are not* thus cleft, proves that *their juices have not been congealed.* It is proper to remark, *en passant*, that even in the cases where the trunks of trees *are split* by cold, *it does not always result in the death of the plant*; although the wood is generally rendered unfit for the purposes of timber. This effect has been, almost universally, ascribed to the expansion which the sap undergoes during the process of congelation; a force abundantly adequate to produce such a result, *provided all parts of the tree were perfectly rigid and unyielding.* As it is manifest that this

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by E. C. Otté, vol. 3, p. 48, Lond. 1851). The maximum natural cold previously recorded, is —70° Fahr.; being that observed by Capt. Back at Fort Reliance in January 1834.

condition *does not* obtain in any tree, I am disposed to think that this opinion is erroneous, and that the effect here referred to *is not the result of the freezing of the fluids*, and, consequently, *is not* a necessary accompaniment of that phenomenon. The following considerations have led me to refer the cleaving of trees by cold, *to the unequal contraction which takes place in the trunk* (usually *after* the complete congelation of its juices) *in consequence of a sudden depression of temperature.*

1. Observations show, that the *oldest and largest trees* are usually those which are cleft, while the younger trees exhibit no such effects. In the account given by I. Evelyn of the effects of the great frost which occurred in England during the winter of 1683–4, we are informed, that "the rifting so much complained of, has happened chiefly among the overgrown trees, especially oaks," whereas elms of only 25 or 30 years standing were untouched (Vide Phil. Trans., vol. 14, p. 559 abridged; vol. 2, p. 153). Now, if *freezing of sap* is the cause of the phenomenon, young and small trees would certainly be more liable to be cleft; both on account of the greater accessibility of their interior parts to the exterior atmospheric changes, and the presence of a greater amount of fluid in their tissues. On the contrary, if the effect is produced by the *contraction* of the exterior layers of wood, we should naturally expect the rifting to take place in old trees, where the heart wood is indurated and, of course, unyielding.

2. The same effect is produced by extreme cold on *dry and seasoned timber* (Vide Phil. Trans., vol. 14, p. 559). At Prince of Wales' Fort on Churchill river, near Hudson's Bay, Capt. Middleton noticed that *trees, joists and rafters* were burst with great noise from the effects of cold (Phil. Trans., vol. 42, p. 157). Near Moscow, the timber work of houses is frequently observed to crack during severe winters (Boyle's Works, vol. 2, pp. 274 et 276, Lond. 1744). Other instances might be cited; but the effects of cold in temporarily widening the small cracks produced by ordinary desiccation in posts and pillars, must be so familiar to every one that farther notice is unnecessary. Is it reasonable to suppose that in these cases the splitting is caused by the congelation of the comparatively small amount of hygrometric moisture which is known to be present in the best seasoned wood? On the other hand, is it not obviously a phenomenon of unequal contraction, precisely analogous to the smaller fissures produced by rapid desiccation?

3. All travellers in high northern latitudes testify that the very rocks are sometimes rent asunder by the intensity of the cold, and that the ground is frequently cleft by the same cause, producing openings many yards long and 10 or 12 inches wide. Such phenomena cannot be ascribed to the expansive power of freezing water, because they occur during mid-winter, and in latitudes where the rigor of the climate is such that the earth is frozen 300 or 400 feet deep (certainly far below the depth of these superficial fissures), long before these effects are observed. They are unquestionably the result of superficial contraction, produced by the sudden application of intense cold, and are precisely analogous to the fissures which originate in clay and mud during the process of drying.

4. Ice itself is frequently known to crack from the same cause. Of this character were the fissures in the ice—some of them *four inches* wide—which M. Erman observed near Bol-Atluimsk on the Obi, and at Posolskoi. He very correctly ascribes them to the cooling and contraction of the upper stratum of ice *subsequent to its perfect congelation* (Erman's Travels in Siberia, Ed. *cit. ante*, vol. 1, p. 331, et vol. 2, p. 218). Such facts are matters of the most common observation in all climates where the cold is of sufficient intensity to maintain water in a solid condition for any considerable portion of the winter. The cracking always takes place during periods of rapid augmentation of cold. To those who may be incredulous as to the *adequacy* of superficial *contraction* to produce the observed effects, it may be proper to state that, according to the experiments of MM. Brunner and Schumacher, the contraction of ice consequent upon a diminution of temperature is greater than that of any other solid body hitherto examined. The former found the amount of linear contraction to be equivalent to 0,00002083 for a degree of Fahrenheit: the latter obtained a still higher result, viz. 0,000029086 (Vide *Ann. de Chim. et de Phys.*, 3d series, vol. 14, p. 369, 1845, as cited in Silliman's Journal, 2d series, vol. 1, p. 117, 1846; also, vol. 3, p. 450 of the same journal). It is well known that glass, in which the coefficient of dilatation is very small, will readily crack by the sudden application of cold*.

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* In two recent series of experiments, independent of each other, MM. Pohrt and Moritz have found the linear expansion of ice for an interval of *one degree* of Fahrenheit's scale, to be 0,0000285 and 0,000028778; fractions nearly ac-

The facts and considerations above detailed seem to point to the unequal contraction produced by the sudden application of cold, as the true cause of the bursting of trees in rigorous climates. Whatever may be thought of the universality of this cause, direct experiments and observations prove that congelation of the sap *per se does not* invariably produce the effect. It appears to us infinitely more probable that the rifting supervenes subsequent to complete congelation of the juices : first, because the *increased non-conducting power* of the woody structures under these circumstances would, of necessity, tend to establish a greater *inequality* of contraction when the frigorific influence began to operate, and therefore a greater liability to rupture; and secondly, because the greater *rigidity* of the interior parts of a tree whose juices are frozen, would tend to bring about the same result upon a sudden reduction of temperature. The reason why some species of trees are more liable to be rifted by cold than others, is probably attributable to a difference in the compressibility of their structures when in a frozen condition.

But it may be asked, if *the freezing of the sap* does not always kill plants, in what manner does cold produce death in vegetables? As this is a point which has been investigated by Gœppert, Morren, Lindley, and others, I do not propose to notice it farther, at this time, than to indicate the bearing of their deductions on the subject under consideration. The observations of MM. Gœppert and Morren seem to prove that the common opinion, that cold acts mechanically upon the tissues of plants, by expanding the fluid they contain, and bursting the cells or vessels in which it is inclosed, is totally untenable; such supposed laceration of the vegetable tissues seldom, if ever, taking place, even when the most succulent plants are frozen and killed by cold. During the process of congelation, each cell of the tissue becomes individually larger, by the augmentation of volume which attends the solidification of the contained fluid; but there is *no bursting*, because the membrane is *extensible*, and, when thawed, the cell recovers itself by its elasticity. The more recent observations of Prof. John Lindley are, on the whole, confirmatory of these conclusions; for although in some instances he found the tissue of the succulent parts of plants lacerated, as if by the dilatation of the fluid

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cordant with the results of Schumacher's experiments (Vide Liebig and Kopp's Report on the Progress of Chemistry, etc., English translation, vol. 1, p. 44, London 1849).

it had contained, yet this result was by no means an invariable concomitant of freezing, and is not essentially connected with the destruction of vegetable life. Upon a careful review of all the facts, Prof. Lindley concludes, "that the fatal effect of frost upon plants is a more complicated action than has been supposed; of which, the following are the more important phenomena:

"1. A distension of the cellular succulent parts, often attended by laceration, and always by a destruction of their irritability.

"2. An expulsion of air from the aeriferous passages and cells.

"3. An introduction of air, either expelled from the air-passages or disengaged from the water during the act of freezing, into parts intended exclusively to contain fluid.

"4. A chemical decomposition of the tissue and its contents, especially the chlorophyll.

"5. A destruction of the vitality of the latex, and a stoppage of the action of its vessels.

"6. An obstruction of the interior of the tubes of pleurenchyma (woody fibre), by the distension of their sides" (Vide Professor A. Gray's Abstract of Prof. Lindley's Memoir in Silliman's Journal, 1st series, vol. 39, p. 18 et seq., 1840).

It will be observed that the phenomena are partly mechanical, partly chemical, and partly vital. So far as the *mechanical* effects are concerned, it is very plain that whatever increases the amount of moisture in the plant augments the liability to laceration of tissues when freezing supervenes. In relation to the *chemical and vital* phenomena, it is sufficiently obvious that the effects of cold must vary with the condition of the fluids in the plant. The well known evil influence of cold in the spring, or after a warm spell in winter, is probably referable to the augmented *susceptibility* which seems to attend the *growing state*. It is difficult to say whether this increased susceptibility to the action of cold is due to an alteration in vital sensitiveness, or to a proneness in the fluids to enter into chemical decomposition*. Possibly both causes may be in operation; but ad-

* It is very probable that the injurious effects of cold upon *growing* plants may be, in part, due to the sudden stoppage of the changes which attend the process of active cell-development. It is reasonable to suppose that any disturbance of this process at such a period would lead to rapid and fatal chemical reactions. The well known fact that, in frozen potatoes, all the starch is converted into dextrine and sugar, shows that important chemical changes may be brought about by the influence of cold.

ditional observations and experiments are wanting to clear up many points relating to the *exact manner in which the death of plants is caused by cold.* Nevertheless, whatever may be our degree of ignorance in relation to these points, it is hoped that we have succeeded in establishing the fact, *that the destruction of life is not an invariable concomitant of the congelation of the juices of plants;* and consequently that they have *not* the relation of effect and cause. Indeed, the proof of this may be considered *twofold : both negative and positive.* For it is well known that many tropical exotics are destroyed by a degree of cold considerably *above* the freezing point of water, when, of course, their sap cannot be in a state of congelation ; while, on the other hand, as I have endeavored to show in this memoir, the juices of other plants are obviously and repeatedly *frozen* without the slightest injury to the powers of vegetation.

The analogy between animals and vegetables seems to be, in this respect, almost as perfect as it is remarkable. A degree of cold which absolutely freezes the fluid contained in their structures, *is not* equally fatal to all plants. As among animals, each species of plant is adapted to endure a certain range of temperature, which determines, with more or less precision, the limits of its geographical distribution. The fact that vegetables are less susceptible to the injurious influence of cold when in a *dormant state,* seems to be a wise and inestimably excellent provision appointed by nature for the preservation of the vitality of the system against the extreme cold of winter. Observations and experiments are yet wanting to determine whether those members of the animal kingdom, which have little or no power of resisting external changes of temperature, are endowed with a like *increased* immunity from the injurious effects of cold during the period of hybernation. On a future occasion, I hope to make this point a subject of special investigation.

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12. Views on the Nature of Organic Structure. By Lieut. E. B. Hunt, Corps of Engineers, U. S. A.

Throughout the whole range of organic existence, both animal and vegetable, there is an evident adaptation of species and individuals to the particular circumstances in which they are found. Nor is it

less true, that all animals and vegetables exhibit a broad general adaptation to the great cosmical and chemical peculiarities of the earth itself. The conditions indispensable for the existence of each organic species are such, that we cannot imagine its vital possibility except in its present astronomical *habitat*. Any great change from the earth's values for gravitation, the atmospheric pressure, the average heat and light, or greatly increased variations in these elements of condition, would prove fatal to all our present species. Nor could these species long survive a fundamental change in the atmosphere, the waters, the vapors, or the soils of the present terrestrial system.

The problem of conditions of existence has an astronomical or cosmical phase. We can conceive a planet with platinum, gold, and silica continents, with a mercurial atmosphere, with elements whose combinations should be a hundred-fold more refractory than ours, and yet with temperature conditions such that its aggregates of inorganic matter should be much the same as we now see. Moreover, we can conceive the existence there of vital organic forms, composed of elements wholly different from those entering organic bodies here, but whose functions should be performed exactly in conformity with earthly analogies.

How vastly unlike in physical features are this Earth, Neptune, and Mars; yet some kind of organic life is, doubtless, actually existent on each. The structure of material forms, fitted to perform organic functions, involves an idea of the highest generality, and is mechanically possible in an infinity of material conditions. Either the organic form or the *habitat* being given or predetermined, an intellect sufficiently capacious could determine the other by *pure computation*. If to this intellect be superadded the power of giving material form or expression to these computed results, nothing more would be necessary for placing any conceivable organic type in its proper home, or in forming for any home its proper organic inhabitant. Man's intellect quite suffices to conceive how the Divine Creative Mind might infinitely vary organic forms in simple adaptation to their dissimilar homes throughout this wide universe. The historic order, in forming for all these varieties the terms of the existing relation between home and inhabitant, has been seemingly, first to arrange the home, and then to construct the inhabitant. The species actually seen around us embody the divine solutions of those particular problems furnished by their specific physical circumstances. The range of variation in these circumstances over the earth's

surface, permitted that immense variety of specific solutions exhibited by the known animal and vegetable kingdoms.

Certainly, natural science is falsely so called, when it ignores the intellectual character of organic forms. An optician who should attempt to investigate the eye without conceiving it as an intellectually composed organ, might labor forever in vain. The single way to comprehend its structure and mode of agency, is by carefully decyphering those ideas which are embodied and expressed in its forms and composition. Its one object is to form distinct images on the retina, and all its parts are composed with a strict view to the accomplishment of this object. A man who should study hieroglyphics, a locomotive, a telescope, a watch, or a book, in a spirit of mental negation and stubborn imperception of their intellectual origin, would not be more absurd, than one who sees no intellectual designing in the eye, the ear, the nerves, the skeleton, the whole organic frame. So far from its being unscientific to make a clear and positive use in natural research of the intellectual character stamped on all organisms, it is simply self-inflicted blindness and deliberate paralysis to ignore those god-thoughts actually embodied in each vital structure. The meaning and design of these structures are not less real than the matter composing them; just as the design and mental significance of a house are equally real with its materials.

A distinct conception of the intellectual arrangement of organic parts, in themselves, in their connections, and in their external relations, gives a clue to the physical nature of organic structures, such as no other view can give.

Inorganic masses of matter have an unlimited capacity to give expression to ideas, either without motion, as in the fine arts, or with motion, as in machinery of all kinds. In a locomotive, for instance, thousands of ideas, first existing only in the human mind, are materially embodied and formalized through this complex arrangement of parts, all of which act in designed relations. Functions of various kinds are performed in harmonious concurrence, exhibiting a partial semblance to vitality. Man's history proves, that were his intellect a hundred-fold greater than it is, machines might be devised which would perform unimagined wonders. Every increase of intellect would give increased capacity to work ideas into material forms. A comparative machinist might, from a machine, infer the mental character and proficiency of its designer, just as a comparative anatomist makes out from the bones of an animal, all its habits.

Any designed material structure reflects the intellect of its designer, and becomes higher in character with each exaltation of the designing mind. Were man's intellect to grow towards an infinite stature, where could we draw a limit to his capacity for giving material embodiment to his most advanced ideas? Would not his ever enlarging mind still clothe itself in material forms of proportionate subtlety in structure, function and design?

But man, in erecting material structures, deals only with masses. He has no power to build up his forms, by using at will, and in succession, *single molecules* of each chemical element. Were man's perceptions and capacities so microscopic that he could work with single molecules, and were his intellect sufficiently exalted, he might build up, in a purely mechanical mode, the exact similitude of any existing organic form, even that of his own body. Infinite intellect having wrought out the ideal of man's body, might thus materialize that ideal by simple arrangement of molecules, without the least change in any one molecule, or the introduction of any new mechanical element. To such a structure, the forces constitutional in matter would give coherence and stability. What more is required for conceiving the physical character of an organism, at a given instant of time?

Another step remains to complete our conception of the whole life of organic structures. What then is growth, and how is it to be conceived or explained?

The ideal of an organism extends through its entire cycle of being. The ideal of an oak is not the mere form of to-day, but that aggregate of formal progressions included between the acorn and the decaying oak. It includes provision for everything needed to insure its normal perpetuity. The history of an organic individual or species must always contain the two great elements of original constitution and circumstantial position. The specific ideal is inwrought and formalized in its constitution, while physical circumstances mar or exalt the development of this ideal. Unless an ideal were framed with a foresight of circumstantial influences, it must soon be frustrated. Now it is very possible to conceive how structures, intelligently composed from simple ordinary molecules, may be made to embody both static and dynamic ideals. The forces appertaining to the ultimate units of matter are quite adequate to work out ideals extending over the changes of a lifetime. In all organisms, the normal changes are progressive, and exemplify the

law of continuity. This continuity extends unbroken through countless generations, always exhibiting strict conformity to mechanical requisitions. The wheel of specific life rolls steadily on, while its points describe the cycloids of individual life. A present structure may be so composed, that the molecular forces acting between its constituent molecules will of themselves work out a long train of predetermined changes. No matter how complicated the system of molecules may be, the determination of the orbit and movements of each molecule is a strict problem in mathematical mechanics, exactly the same in principle as that of planetary or projectile motion. The vast intellectual difficulty of the discussion does not affect its principle, nor would it obstruct a full predictive insight by a mind of sufficient grasp. Those differential worlds, whose integrals are seen in organic masses, move on, planet-like, in the round of their mechanically determined orbits.

That intellect which was large enough to idealize man, was, doubtless, large enough so to construct his body that the constituent forces of its component matter should operate the observed renovation and progression of its parts. In framing the ideal of which man is the embodiment, not only is it conceivable that the designing intelligence inwrought every mechanical essential for the physical functions of a full grown man, but that a definite physical provision for all the changes in the whole cycle of his being was incorporated in the structure of his body. The individual cycle of growth, and the physical history of our race, from its beginning far into the unenacted future, may have been structurally embodied in the frames of our created progenitors, and in the circumstances of existence surrounding them and their posterity. Thus, too, by direct design may the body have been made the structural depository of all those harmonies which alone can fit it to become the physical home of the spirit which inhabits it.

This mode of viewing organisms furnishes a hint as to instinct. A divine ideal would contain provision for all stages in the development of the individual. The activity of animal faculties is closely connected with, and mainly controlled by, physical structure, which, in all stages of development, must embody the necessary conditions for continued existence. In designing and framing an animal structure, the means for stimulating all the faculties required at each stage of its existence would be introduced in its bodily constitution. Thus the operations, usually called instinctive, would result from a

predetermined structure, specially designed to stimulate the particular faculties exercised. The materialized ideals would determine which faculties would be most active at each stage of growth, and in each species of animals.

The views now presented are based entirely on the conceptions of matter and its constitutional forces, which inorganic masses constrain us to adopt. It cannot be too distinctly stated that every mechanical idea is outraged by the common conceptions of a peculiar "vital force" or "organic force." The term "force" has a definite meaning when used in mechanics, but no one can define a vital force. It is too mysterious, fickle, evasive, and illegitimate to permit a clear conception or definition. When an organic process is not understood, a vital force is usually summoned to remove our ignorance out of sight. Never was anything more purely hypothetical. Its parent is ignorance, smothering truth and investigation its office. Until some shadow of evidence is presented, that a special unmechanical force, peculiar to organic bodies, really exists, we are in duty bound to abjure this convenient, this elastic figment of a vital force, which, "having no law, is a law unto itself." Though the complication of organic structure may forever prevent a strict mechanical analysis of organisms, we are not thus authorized to hypothecate a new force for convenience in cloaking ignorance. So far as we know, no molecule is ever moved, except by a real mechanical force, nor indeed can be, on account of its inertia. Though our muscles move in obedience to nervous impulses, the agencies applied are doubtless wholly physical below that point, so wholly mysterious, where the nerves centralize into one subtle thread of connection between the spiritual and material part of man.

This glancing into the depths of organic structure is no transcendental flight beyond the actual. Our appeal is only to that intelligence actually exhibited in formalizing the masses which compose organic bodies. Let our conceptions but extend the sphere of this intelligence to the intimate constitution of each organic mass, and we shall find a new light thrown on the whole nature of organic structure. It is true that every organism, on this view, is an embodiment of more subtle intelligence than all mankind can boast; but he must have studied nature quite in vain, who has not seen compulsory evidence that the organic architect is indeed great.

13. On some Special Analogies in the Phenomena presented by the Senses of Sight and Touch. By Prof. Stephen Alexander, of Princeton.

Prof. Alexander first noticed the well-known deception produced by placing a pea, or other small body, between two of the fingers, previously crossed — the pea appearing double; and mentioned and illustrated several other deceptions, such as may be obtained,

1. By holding a book between the hands, the palms turned outward, the arms being previously crossed : the edge of the book appears, in this case, to be bent at an angle.

2. By passing the one hand over its corresponding shoulder, and the other under the arm belonging to it, and holding a book between the hands thus placed : the edges of the leaves of the book appear to the one hand to be a continuation of the surface of the back of the book.

3. By folding the tongue back upon itself : the portion thus placed appears to be a foreign body laid upon the tongue, except that the sensibility of the part thus turned is still perceptible.

4. By turning the tongue up edgewise, and then placing it against the teeth : the teeth appear to be placed in an inclined, instead of a vertical position.

Prof. Alexander then stated the proposition that the duration of impressions produced upon the organs of touch, like those upon the organs of vision, is not instantaneous, but continues for some short period of time. This was variously illustrated :

1. A square bar, rapidly revolved, will appear to the fingers to be a round file.

2. Central sections of the cylinder, cone and sphere, will appear in each case like the whole solid.

3. A circular arrangement of round-headed nails driven into the face of a circle, but excentrically situated on that circle, will appear to have an excentric movement on the face of the circle, as the latter turns about its own centre.

4. A spiral arrangement of the nails around the centre of the circle will appear to unwind when the rotation of the circle is in one direction, but to coil itself up more closely when the direction of rotation of the circle is reversed.

Other arrangements were exhibited, and some merely indicated.

Prof. A. stated, moreover, that when two plates of metal, unequally heated, had been made to revolve rapidly and come alternately in contact with the fingers, the result seemed to be a sensation of an intermediate temperature. He also indicated a device by which one part of a rotating body might be felt to be hard, and the succeeding portion soft; but the result of a rapid rotation of the whole would probably present the sensation of an intermediate constitution. Lastly, an indication was given of a plan for an apparatus by which one finger might be subjected to a uniform, and another to a variable pressure; but by a rapid rotation, the successive impressions of the variable pressure might be equalized.

Prof. Alexander, in conclusion, briefly adverted to the experiments of Weber, "*De pulsu, resorptione, auditu et tactu.*"

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14. On the Relation between Erect Vision and the Inverted Image on the Retina. By Prof. W. W. Clark, of Albany.

The question as to why we see bodies erect while their images on the retina are inverted, has for a long time attracted the attention of philosophers, and several theories have been presented to explain the phenomenon.

Sir David Brewster, many years since, presented a theory in which he supposed the eye to possess a power which he calls the power of "visual direction"; and that by means of this, the mind takes cognizance of the general direction in which the rays of light come to the eye, and thus of the object itself in an erect position. The details of this theory, and the objections to it, have been so fully discussed in our elementary works on physiology and optics, that all are probably familiar with them.

Another view which has been presented, is, that at first the eye presents all nature to the mind of the child in an inverted position, by means of the inverted images on the retina; but that in a short time the mind learns the fallacy of these impressions, and, becoming accustomed to the inversion, is unconscious that they ever appeared otherwise than erect. That this theory is not a correct expression of the relations which exist in nature, I think will be made evident by what follows.

It seems to me that the great difficulty which the authors of these theories have found in arriving at correct conclusions in this matter, arises from the fact that they had not clearly defined in their minds what is the natural province of the eye; that is, how extensive, and of what kind is the knowledge which the eye, unassisted by the other senses, can convey to the mind; and, accordingly, I will first proceed to examine this question.

That the mind has no connection with external nature except through the senses, is, I believe, at present undisputed; and it is equally well established that the mind can obtain directly no knowledge of external objects, until they come *in contact* with the nerves or organs of touch. This being true, we see that the light which enters the eye and strikes the optic nerve, can of itself abstractly convey no idea of the object from which it emanated; since the mind takes cognizance of light only, that being the only thing in contact with the nerve. The first ideas, then, which the mind receives through the eye, are those of the *form* and *color* of the image which is formed upon the retina; but by means of the unassisted eye, the mind could have no knowledge that there was any thing external to which this colored image corresponded. The province of the unassisted eye is to convey to the mind *only* the impressions of the form and color of the images which, by the agency of light, are formed on the retina; it being entirely unable to give rise to any idea of distance or magnitude, or of any thing else external.

From this we see that until the eye has been educated by the sense of feeling, there can be no such thing as right or wrong side up to the images on the retina, since the mind is yet totally unconscious that these images bear any relation to objects without. The first time, however, that the hand comes in contact with an object, at the same time that there is an image of it on the retina, the mind recognizes the correspondence between the image and the object, and of course learns what part of the image corresponds with its top or bottom. After the mind has once learned these relations, it will necessarily ever after recognize that the part of the image which is really *(but not at all known by the mind to be)* uppermost in the eye, belongs to the bottom of the object, and *vice versa*.

Whatever, then, affects the retina on one side of the eye, will *seem* to affect it on the opposite side. This is beautifully illustrated by a phenomenon which attracted my attention for the first time about six years since. At this time I was lying on the sofa in the evening, with

my eyes turned towards the flame of a lamp which stood at a little distance from me. One of my eyes being closed, and the other partially so, I noticed that two *streams* of light appeared to come to the partially closed eye; the one from above, and the other from below. In examining the phenomena which these streams presented, I was surprised to find that when I brought my finger down from above, so as to intercept the light from coming to the eye, the lower stream was invariably intercepted first. I explain these phenomena by referring the appearance of the stream of light to the light which shone through the eyelashes, and thus upon the retina in the shape of a long pencil of rays which seemed to converge at the flame from which they emanated. It will be readily seen that the rays which pass through the upper eyelashes fall upon the upper part of the retina; and that in accordance with the above theory, the mind will suppose them to come from below, and *vice versa*.

There is another phenomenon which all have doubtless observed, which is also beautifully explained by the above theory. Whenever the finger, or any other hard substance, is pressed against the ball of the eye, the sensation of a ring of light is produced; and what is the most peculiar, is that this ring always appears to be on the side opposite the point of pressure. Although this appears strange at first, it is still exactly what we should predict by the above theory; that is, the effect on the retina being produced on one side of it, that effect will appear to the mind to be on the opposite side of the eye.

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15. On Daltonism, or Blindness to Particular Colors. By Prof. M'Culloh, of Princeton, N. J.

[*Not received.*]

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16. Relations of Embryology and Spermatology to some of the Fundamental Doctrines of Physiological Science. By Dr. W. J. Burnett, of Boston.

When the influence of the study of organic chemistry and microscopy was beginning to be felt in natural science, the prospect was held

out that soon we should understand fully the intimate and primordial relations that exist between the organic and inorganic world.

This was promising too much, and a disappointment has necessarily ensued; still, the quite thorough prosecution of these studies has, I think, produced two distinct results or opinions. With the chemists, the tendency has been to regard the movements of the organic world as simply the results of modified chemical forces : in other words, that matter and chemical power include the phenomena of life. With the microscopists, on the other hand, the tendency has been to consider organic matter as endowed with a power above and beyond these others; and that we are to recognize, in the expressions of life, *vital* as well as chemical forces. I believe this is the tenor of all carefully pursued microscopical studies. With chemists, vitality is always materialized; with microscopists, it exists as an entity above and beyond matter, and may be considered as its thought or idea.

There is a great difficulty in investigating subjects of this kind, because we are in constant want of requisite data from which we can safely draw conclusions. But since my attention has been called to the microscopical study of developing forms, or, in a word, to organic atoms, I have been induced to adopt the opinion that, beyond and isolated from matter, there exists not only what is termed a vital force, but ideas and thoughts.

I propose to illustrate these views by a consideration of some results at which I have arrived in a new and rather peculiar department of microscopy, viz. Spermatology, or that which relates to the intimate nature of the spermatic particles. But that these may be better understood, it is necessary that I should refer for a moment to the present conditions and relations of this branch of science.

In embryological studies, we commence with the simple ovarian cell, or even still further back, with its nucleus : this we trace upward until it has grown to a perfect cell. We then watch the endogenous formation of the cells within it, until it is a great compound cell, which is called the ovum; and then we observe the modification of its contents into a symmetrically shaped body, which is the new being. Now, throughout the animal kingdom, not only has this primitive ovarian cell the same material aspect, but the same is true of the great compound cell or ovum. This, then, is the fundamental point in all development; and from the most careful examination with the highest and best microscopical instruments, we are unable to perceive why, for instance, one cell should give rise to a spider,

while another, appearing exactly like it, should give rise to a bird. We can reason only from what we know; and if in studying the ultimate atoms of two different portions of matter, we can detect no difference with our present instruments, we certainly have a right to affirm that these portions of matter are identical in physical character. We have a right to infer, also, that that power which prompts each cell in the above instance to its ulterior condition, viz. the production of a spider or a bird, has, in the cell, no material expression by which it can be determined, but that it resides as a simple, pure force or individual entity.

Some may call it a dynamic power inherent in the cell, but this is only expressing the same in different terms. We have, then, in a single cell, the complete idea of a bird existing not as a material condition, but as a pure individuality; which last is shown by the fact that the idea is not that of simply *a* bird, but *one of* distinct characters, which here exist *in* thought or type so minutely as to comprise even the color of the bill and length of the feathers.

This view is well supported by all facts of hybridization; for when allied species unite, and there is in the offspring a union of the characteristics of each, this last could have occurred only at the time of fecundation, when the ovum was merely a compound cell, and when it must have possessed all the specific characters of the female. In this relation I cannot do better than to quote the words of the profound Müller. He says: "The simple embryo, which consists of a granular, shapeless substance, is to be regarded as the potential whole of the future animal, supplied with the essential and specific force of the future animal itself." It may be urged that this idea is quite indistinct, and that we are deviating from the true method of physical investigation, by affirming that the forces of a bird, for instance, exist as such before the material organs by which they are to be expressed have been formed; but this objection is not true or valid, not only because it is based on a mere opinion of the nature of organic matter, but also because it is contrary to many facts; for we do have *attempts* at the expression of individual forces long before the organs by which they ultimately find their complete exhibition are developed, or even when they are never developed. Let me therefore add, that if careful embryological studies teach anything in this connection, it is that each healthy ovarian cell contains the potential whole of an individual like the parent, and which is constantly seeking its complete development, to be attained only by the

coöperation of a corresponding element of the opposite sex; and that the type of form ultimately expressed, being the outward exhibition of inward forces, cannot long suffer deviation without destruction, and is fully as permanent as the individual itself.

If we are not allowed these forces in organic forms, we certainly never can rise above mere material forms : in fact, there would appear to be no reason why the ovarian cell of a bird should not produce equally as well a mammal. But these relations are not equally clearly understood by all; and some whose attainments should have led them to think differently, have so failed in their appreciation of them that they have regarded the ovum as simply an organic molecule, thus putting an end to all dispute in their own minds as to the doctrine of spontaneous generation and epigeneses.

I have thought, if anything were wanting in embryology to render such views complete, the complement could be fully found in its counterpart science, spermatology; and I have therefore taken it up in that connection.

What embryology is to the female, that spermatology is to the male. In a histological point of view, the process is the same in each sex. In the one, you have a simple cell passing on in development to a new being; in the other, you have a simple cell passing on to the development of an organic vitalizing particle, which is the prototype of that being. There is, however, this difference, which should be remembered : it is, that in embryology, the new individual form is the result of the coöperation of the two sexes; while in spermatology, it is the result of one. Therefore our philosophical studies in the latter begin for the most part where, in the former, they have ended; for the spermatic particle is the material expression of the male, both generally and specially : it is, in fact, the male embryo. The course of study by which this important truth has been ascertained, I have treated of in another place.

We here begin with the simple testicular cell, the growth of which we watch until it has become the great compound mass, the parent sperm-cell, in which, by a modification of its contents, are developed the spermatic particles. These organic particles are the true and only fertilizing agents in the process of fecundation, in which, experience has shown, that they do not merely fertilize—that is, light up a pile before all ready to burn—but they coöperate and furnish conditions essential to the perfect result. We know very well, that, in the higher animals, where often there is a dissimilarity of form

and external appearance of the two sexes, the offspring not unfrequently have all the characteristics of the male, even down to minute points.

In our own species, we are daily observing how correctly and faithfully the child often inherits, not only the physical, but also the peculiar mental features of its father; and too often, too, do we see in the same inheritance a variety of disease. Now, experiment has shown that, in this process, a single spermatic particle only is required, and that there is no incorporation of its substance with the ovum, but that the whole is accomplished by mere contact with the periphery of the ovum. We have, then, in a word, the organic particle of a male, microscopically minute, which, by mere contact, and without any material loss, transmits to the ovum not only the potential whole of the male generally, but particular mental and physical characteristics. It must be, therefore, that in a minute particle of matter there be hidden often not only the silent thoughts or prototypes of poetry and art, but also of peculiar love and affection.

It cannot be otherwise, and is no more difficult to be comprehended, nor is the idea less beautiful in suggestion, than that these same *thoughts*, when matured in after life, with daily expression, should occupy the strange relation which they do with the material substance of the brain.

It is argued that the idea of *transmission* of force is quite unphysical; because, say the chemists, that only which is substantial can be communicated. But this I consider simply reasoning on the ground that a condition in nature is impossible, because it eludes our means of study and observation. But if we are led to take views like these, we must not at the same time suppose that all spermatic particles are, like ova, identical in physical appearance : for the spermatic particle is not the analogue of the ovum, but of the new being; and as in the latter, so in the former, we find well-marked differences of type. Still the physical characteristics of this particle are not always expressive in any way of the individual of which it is the prototype, and both the similarities and dissimilarities afford a fine argument on the point we are now discussing. For instance, I can show a spermatic particle of a duck, which, as to external appearance, as well as by micrometrical measurement as to length, breadth and thickness, cannot be distinguished in any way from another of a reptile : yet in the one is embodied the idea of a bird, even to the color of its plumage; while in the other is embodied the thought of a reptile.

But there is another point from which I wish to view this subject, and which takes us still further back in histology. I refer to the type of the spermatic particle itself. In any animal, there is but one form of this minute particle; and from it there is never any variation, any more than there is in the elimination of the embryo from the female; and exactly as there resides in the ovum the thought or idea of a certain-shaped future embryo, so it may be considered there exists in the sperm-cell the thought or idea of a peculiarly formed spermatic particle. This may be safely inferred from the uniformity of results with which we meet; but in the course of my studies in this direction, I have met with phenomena which illustrate this point in a striking and beautiful manner, and at the same time exemplify to us how wondrous and how certain is this type-power in the ultimate attainment of its object.

With one or two preliminary remarks, I can show what I here mean. In the development of a spermatic particle, you have, as I have before said, the parent sperm-cell, which is a large cell filled with smaller ones, each of which is nucleated. The direct formation of the spermatic particles here begins to take place, and it occurs in two general modes : the first is called *the special cell;* the second, *the fascicular* mode of development. But in some of the rodentia, I have observed, apparently even in the same animals, *both* modes of development; in fact, two parent sperm-cells, side by side, in one of which the spermatic particles are eliminated by *the fascicular*, while in the other by *the sperm-cell* mode. The results were exactly the same, and the particles of each could not be distinguished from each other. We have here, then, a single identical result from two dissimilar processes. This fact stands for a great deal; for it would appear that whether the development takes the right or the left-hand road, the type-power brings out always the same result. It is destined to get its expression some way, and certainly appears to argue that there are dynamics above and beyond material forms.

When an artist portrays in sculpture the outward forms of a beautiful human face, we can have no doubt that the image of the same lived in his imagination before the material work was commenced. But when the same artist portrays in painting also the same beautiful face, with exactly the same lineaments, this affords an additional evidence that the conception was pure and distinct, and lived as such in his mind; and it mattered but little whether it took

this or that outward visible form, for the expression of the creative thought would always be the same.

Exactly so is it with spermatic and embryonic typical forms. The idea or thought behind each, proves its individuality by the uniformity of its expression : it matters but little or nothing whether gained in this or that manner.

In conclusion, let me say that I consider a thorough appreciation of these doctrines, of what may be termed higher dynamics, quite essential to the progress of physiological science; and I cannot see why men should retard it by a series of detailed explanations which really obscure the matter. Why not recur at once, for instance, to vital force, for the explanation of certain phenomena; and then we shall have a point of departure, the determination of the laws of this *vital force* remaining for our future study.

Liebig says : "As soon as physiologists meet with the mysterious vital force in any phenomenon, they renounce their senses and faculties; the eye, the understanding, the judgment, the reflecting faculties, all are paralyzed, as soon as a phenomenon is declared incomprehensible." I do not consider this true; or even if it is partially so, it applies to the chemist as well as to the physiologist. It is useless to cavil about these matters. We must have terms to express phenomena. In the natural world, one thing appears almost as mysterious as another. Even if we do reduce everything to mechanics and chemical action, is the matter then made more clear? Do we know anything about the fundamental principles of mechanics or chemistry? Or is a *vital force* more mysterious than is *motion* with the one, or *affinity* with the other? Is it more comprehensible that an elective affinity should exist in inorganic particles, than that animal types should exist in organic ones? Of all these matters, we know nothing except from their objective phenomena, and we cannot indeed do less than to show a consistency in their recognition.

E. ETHNOLOGY AND GEOGRAPHY.

I. ETHNOLOGY.

1. Description of Samples of Ancient Cloth from the Mounds of Ohio. By J. W. Foster, U. S. Geologist.

I avail myself of this opportunity to exhibit to the Association, samples of cloth procured from two mounds in Ohio, nearly a hundred miles asunder.

As far back as the year 1838, I procured from a person residing in Charlestown, Jackson county, Ohio, several fragments of cloth which he had taken a few days previously from a low mound in that vicinity. It was found near the original surface, enveloping several sets of copper rings, and for the most part was so far decayed as to exhibit only the textile structure, but some of the fragments were in a good state of preservation.

This fact was so novel in itself, and so at variance with the prevailing ideas as to the degree of civilization and the knowledge of the arts among the mound-builders, that I hesitated about making it public; fearing that it might have been a modern substitution, and that by publishing, I might be the means of propagating error. I therefore refrained, trusting that additional evidence might be brought to light. I have now that evidence in my possession. Within the last six weeks, I have received from Mr. John Woods, a gentleman who occupies a high official station in Ohio, and whose statements are worthy of all credence—additional samples, accompanied by a description of the circumstances under which they were found. I beg leave to quote the letter of Mr. Woods entire.

COLUMBUS (Ohio), *July* 5, 1851.

Dear Sir: I have handed to Dr. Ide a quantity of the charred cloth which I dug out of a mound on the west side of the Great Miami river, in Madison township, Butler county, O. The mound is about two miles north of Middletown.

The mound was originally about twenty feet high: when I first saw it, thirty years ago, it was probably sixteen feet high. It was covered fifty years ago with large forest trees, as I am informed by the old settlers.

The Cincinnati, Hamilton and Dayton railroad runs through one side of the mound, half of which has been cut down for the track. The workmen informed me that they had found an arrow, and a considerable quantity of charcoal, cloth and bones. I took a shovel, and after removing the earth so as to be sure that I was down as far as the formation had been disturbed, I took out several shovels full of earth, coal and cloth : the charcoal seemed to be that of oak and sugar-tree. Dr. Ide will transmit to you a larger quantity of the cloth than I can send in a letter. I have preserved it in the condition in which I found it in the mound, as nearly as practicable. The workmen said they had found pieces of cloth connected with tassels, or ornaments of cloth. I have retained about half the quantity which I took out of the mound.

About ten feet from the top of the mound there was a firm, compact stratum of fine clay, about an inch thick, which appeared as if it had been burned until it was red; under this, near the middle of the mound was another stratum, of a beautiful fine, yellow or cream-colored clay, entirely different from any in the neighborhood. Under this stratum of yellowish clay I found the charcoal, cloth and bones; they were so loose that I could almost with my hand sink down the shovel eighteen or twenty inches. The bones which I found were few and small. I did not understand that any large ones were found. Very little earth appeared to be mixed with the coal and cloth, which were evidently in the position in which they had been placed when buried and covered up. The charcoal appeared to be on the outside of the cloth, which was frequently in thick folds; half a dozen thicknesses being together.

One-half of the mound is yet standing at the side of the road; and if I can find leisure, I will make further examinations. The stratum of burned earth and yellow clay did not extend over the whole mound, or through it, but occupied only five or six feet in extent, so far as I discovered.

The only question which arises in my mind, as to the time when the charcoal and cloth were deposited in the mound, is whether the mound, erected by an anterior race, may not have been made a burying place by the Indians who existed here when America was discovered, and that they had wrapped their dead in the cloth; and, after partially burning the bodies, or the bones and cloth, had covered up the fire. I thought of this question at the time, and was careful to examine, as fully as was then practicable, the condition of the earth around and over the relics which I dug out; and it appeared to me that the original formation could not have been disturbed after it was placed in the mound.

Very respectfully your obedient servant,

J. W. FOSTER, Esq., *U. S. Geologist.* JOHN WOODS.

There is no evidence that the North American Indians possessed the art of spinning and weaving, when first known to the white man. An art so conducive to the comfort and convenience of man, when once acquired, would not become lost : nor would it be rational to infer that this cloth was a European fabric obtained by the Indians, substituted in the mounds, within comparatively recent

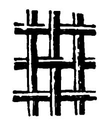
times, for the following reasons : In the Butler county mound, the semi-stratification, described by Mr. Woods, indicates no disturbance.

The material of this cloth is not such as a civilized race would manufacture to traffic with a barbarous one; it being more costly than wool, and less adapted to the purposes of clothing.

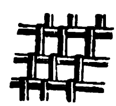
The texture of some of these samples could not have been formed in an ordinary loom, but is undoubtedly the result of handiwork.

These facts have an important bearing upon the ethnology of the people by whom the mounds were built. They indicate a higher degree of civilization, and a greater progress in the arts, than had been attained by the Indians when first known to the white man. They go far to dissever the present race of Indians from the mound builders; a laborious and intelligent people, who have left abundant memorials of their existence from the confines of Lake Superior to those of the Gulf of Mexico.

The fabric in both samples appears to be composed of some material allied to hemp, but less readily recognized in the charred samples than in the others; and the separation between the fibre and the wood appears to have been as thorough and effectual as is accomplished at this day by the processes of rotting and heckling. The thread, though coarse, is uniform in size, and regularly spun. The texture in the specimens from Jackson county, as well as in some of those from Butler county, is formed by the alternate intersection of the warp and woof;



but in others, from Butler county, the weft is wound once around the warp, thus:



a process which could not be accomplished except by hand.

I see no reason to doubt that these textile fabrics are the work of the mound-builders. The art of spinning and weaving was practised by the Peruvians, when their country was first invaded by the Spaniards; and samples of cloth, and the distaffs on which the thread was spun, are associated with the oldest monuments. In corroboration of this, I quote the verbal statement to me of Mr. E. O. Carter, a gentleman who passed several years in that country, and personally assisted in the exploration of many of the remains of antiquity. Thus, at Pachacamao, thirty or forty miles from Lima, where stands the Temple of the Sun, there are numerous remains of

walls, built of adobes, or sun-dried bricks, indicating the site of a once large and compact town. In the *huacas*, or burial places, are found numerous mummies in a sitting posture, wrapped in many folds of cloth, with an exterior covering of coarse matting. It is composed of a regular warp and woof, the thread being twisted or spun, and is often wrought in variegated patterns. The fabric consists of the wool of the lama or alpaca, and perhaps, in some instances, of cotton, which here grows spontaneously. In this connection, it is not unusual to find spindles, with the yarn upon them, which are sticks, nine or ten inches long, terminated at one end by a button : also, various utensils, indicative of the occupation of the deceased; and in some instances, personal ornaments, consisting of gold and silver, such as chaplets and bracelets. Articles of pottery, filled sometimes with corn and sometimes with cotton, are abundant.

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2. On the Aborigines of Nicaraugua. By E. G. Squier, of New-York.

[*Not received.*]

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3. On the Distinctive Characters of the Indians of California. By Dr. J. L. Le Conte, of Philadelphia.

Much has already been written respecting the races of Indians in the western parts of America, and I can scarcely hope to add anything new to what has already been said concerning them. Yet having an opportunity of seeing many tribes not usually visited by travellers, I have thought that it might be useful briefly to give my observations on the distinctive characters presented by these Indians.

Should these observations correspond with those already made by others, they will be interesting as having been derived from a different source : should they not accord, they may perhaps give some further evidence on the much-vexed question of the origin of the race.

The physical appearance of the Californian Indians has already been described so often, that there is no use in repeating the description here. I will merely say, that the few Oregon Indians I have seen did not differ in any essential character from those of California; nor did the sea-shore tribes differ from those of the Sierra, or the valley of the Colorado. My notes are principally derived from observation of the latter tribes; for the reason that I remained longer among them, and because the absence of clothing in these tribes permitted me to make the requisite comparisons more easily.

The special difference between them and eastern Indians consists in the greater extent of face, with smaller and narrower cranium; a less decided obliquity of eye; a greater flatness of nose, dependent on a greater breadth of the alæ, and a less firm cartilage; greater protrusion of lips, and a more pointed chin. The last is a deceptive character, and may be produced simply by a greater expansion of the face below the eyes. The color varies, being much darker in some tribes than in others, and is usually much enhanced by their dirty habits, as they never wash any portion of their bodies, except in summer, as a relief from the excessive heat.

The other characters more clearly separating this from allied races, are, the greater abundance of hair on the body, many of the males having quite as much hair on their legs as is common in our own race; they have also much more hair on their faces than other Indians, and always have hair in the axillæ. These characters are not found in the females.

The differences in form between the sexes are more apparent than in any other race. The males are almost always slender and well-proportioned; while the females are short, broad, and entirely destitute of all symmetry. Nor is this difference owing to the hard work which they are obliged to perform, for they do not labor more than is usual among savages; and, moreover, the same difference in form exists among the peons of the ranchos, where neither sex performs any work at all.

The difference noticed by Major Emory in the form of the nose of the two sexes in the Maricopas, does not appear equally constant. Among the Yumas, there are many females with straight noses, while many males retain the very decided snub form which characterizes both sexes when young.

The mammæ of the young females are more acutely conical than those of any other women I have seen.

In psychical character, these nations show still stronger differences from the eastern tribes. Quiet and submissive, the nations living with the whites have assumed a servile condition, which under no circumstances could have been impressed on the eastern Indian. Impertinent, cowardly and treacherous in a wild state, they seize every opportunity to molest and impose on weak bodies of emigrants; while in the presence of those whom they fear, they exhibit the most abject submission. Never coming to an open attack with the whites, they rely only on treachery, and their unsuspecting victims are always destroyed with clubs.

Another fact, which must be considered as an indication of a psychical peculiarity in the western tribes, is the prevalence of the sounds *khl*, *kl* and *tl* in their languages, recalling at once in their softer forms the civilized languages of Mexico. These sounds occur also in the Oregon languages, becoming more harsh in the most savage tribes : among them, too, is found a set of guttural and glottal consonants of the *k* series, which are entirely wanting in the eastern tribes. It is by such analogies as this, rather than by any affinity of words, that we must be guided in our researches among savages. In languages formed like those of Indians by isolated families, and subject, from the want of literature, to continual change, but little resemblance may be traced between the words of different tribes. Where, however, the mental constitution of nations, when civilization has not interfered with the instincts of nature, is similar, the manifestation of this similarity will be noticed in the occurrence of characteristic sounds in their languages.

The number of articulate sounds is but small, yet the modifications are very numerous. Among the nations capable of intellectual advancement, in the slow progress from barbarism to civilization, the harsher sounds are eliminated, and higher consonants of the same series are substituted. The language thus preserves certain characters of the class to which it belongs, although it may have been so modified that its original form may only be detected with great difficulty. Thus the harsh spasmodic sounds of the Africans are not the gutturals of the American Indians ; nor are these the same as the uncouth sounds of the Australians.

The Mexican language, highly cultivated though it was, and containing no articulate sound not existing in our own tongue, yet retained a polysynthetic structure as its continental character, and an excess of the *tl* sound to aid us in tracing the zoological division in

which it originated. This is the only mark that it retains to show its origin, and this is a sound absent in the languages of the Atlantic tribes. We are therefore led on psychical grounds to class the highly cultivated Aztec with the barbarous tribes of western America.

As a curious analogy, illustrating the psychical relations existing between the Mexican nations and the western tribes, I will mention that on some highly sculptured pipes procured at Nisqually by Dr. PHILLIPS, U. S. N., I was surprised to see human figures in very distorted positions, as if crushed down with immense weights, precisely like those figured as occurring at Palenque, Uxmal, and other cities of central America, as basal ornaments of columns.

Now, although the comparison between a stone pipe and these wonders of original architecture may be considered trifling, yet I view this similarity of æsthetic ideas among nations having no connection with each other, and separated by such a lapse of time, as of considerable interest. If rightly viewed, this fact should teach, as will the relations of language already mentioned, that beside the spiritual similarity which stamps itself on the highest as well as the lowest form of humanity, there are still more special resemblances which are confined each to a particular race, and remain impressed on that race in the depths of barbarism, as well as under the light of intellectual culture.

It is difficult to understand why this view may not be extended to those still more spiritual ideas, which are embraced in the term tradition; the material resemblances in the ornaments of distant tribes being founded on the accordance of principles of harmony and beauty, depending purely on similar organization of the soul of the tribes in question, why should we deny to the still higher attributes of their nature, a power of expressing themselves also in a similar manner, without any direct transmission of ideas from one to the other. Why should not the accordance in the fundamental observances and doctrines of some of the most different primeval religions be ascribed to this spiritual resemblance, rather than to the remnants of a primeval revelation, of which there is not a single witness, either historical or traditional.

By viewing traditions in this manner, we are again deeply impressed with the fundamental, incorporeal unity of the species; while we perceive still more prominently, in the difference between classes of traditions, the original organic difference of races.

My object, in the present remarks, is to show that the investiga-

tion of these traditions must be subjected to scientific analysis and criticism, before they can be allowed to give us any certain data concerning the early history of our species.

II. GEOGRAPHY.

4. On Deep Sea Soundings. By Lieut. M. F. Maury, U. S. N.

[*Not received.*]

5. Proposal for a Trigonometrical Survey of New-York. By Lieut. E. B. Hunt, Engineer Corps U. S. A.

The importance of obtaining accurate delineations of the leading geographical features of this country is so obvious, and so generally conceded, that it would be superfluous here to elaborate arguments in its proof. Geography is no longer content with the vague information of first reconnoissances, but demands the aid of accurate methods and the most perfect instruments. Geodesy, topography, hydrography are indispensable handmaids to any geography worthy a civilized nation. In a country like ours, where so many physical features are new and interesting, it is particularly needful to obtain an accurate representation of the multiplied accidents of ground. There is a grandeur and sublimity even about many of the characteristics of our broad territory, which may well make us proud to picture, in topographical language, all the natural features of this home of a nation now great, and soon to be the greatest.

A nation's home, its mountains, valleys, rivers, lakes, climate and productions, enter so largely into its history and very vitality, that a great importance must necessarily be attached to accurate knowledge of all these particulars. The geography of a country contains a prophecy of the history of that train of nations which, in turn, act their parts on it as a stage. The mountains of Switzerland and Scot-

land foretold, ere man's creation, the histories of a Tell and a Bruce. And here we are in a new world, a world of wonders, over whose surface are written these cryptic prophecies of the ever-widening vista of history that is to be. Where is the prophet who will interpret these hieroglyphics? Philosophers are all prophets within the sphere of their knowledge, being made such by their knowledge. Let the land and water of our country be truly delineated, and we may then forecast the future of our history. This forecasting should not be in a spirit of vague wonder, but with an intelligent purpose to regulate and rightly act our normal part in history. The social and political prospects of our country are so commingled with geographical influences, that in the highest sense is it important to become familiar with the features of our wide territory.

This country is eminent for its internal improvements, and it is evident how great would be the advantage of accurate surveys in connection with this rapidly growing system. How much accurate maps are needed, every one must have felt who has travelled through the common roads of the country. The vicinity of every considerable town should be so represented, that a stranger can understand the network of highways around it. In the thousand ramifications of common social life, the convenience and use of accurate maps is felt. Millions of miles are needlessly travelled, for the want of proper maps.

Surveys of the geographical features of all the States in the Union, with the exception of Massachusetts and perhaps New-Jersey, are now and must ever remain eminently needed until they shall be actually made. It is evident that the time is not distant when these will be emphatically called for by all the combination of needs which require them of every scientific or civilized nation. They will be made; and why not soon enough to give us the benefit of their results? It is certain that if we are among those nations alive to the power and benefits of the sciences characterizing civilized society, the States of this Union must in turn be surveyed with that nice accuracy which geodesy now demands and furnishes. We may then best enter at once on this labor, so that the benefits as well as the labor may be ours, while those who follow us may enjoy the benefits without the labor.

It has seemed to me, in view of these and many other considerations which need not here be adduced, that it is fully time for New-York to undertake an accurate geographical survey of her whole

territory. With a population of three millions, with wealth and resources making her not unworthy the name of Empire State; with an enlightened legislature, which has ever nobly favored science; with thousands of highly educated citizens, who would fully appreciate the advantages of accuracy in its geography and topography; with these, and many other worthy stimulants for doing its high duty, it has seemed to me very proper here to propose, in serious earnest, that a beginning should be made. To nearly every member of this Association, I am convinced this subject need but be named to ensure hearty appreciation. In this Association, now convened in the capital of the very State in question, I see the most proper body for actively urging the claims of this proposal on the Government of New-York. Here, if anywhere, is a fair and honest tribunal for the trial of this proposal, and one to whose verdict respectful attention will surely be conceded. Permit me now to say, that when science fairly urges her claims in the spirit of honest, true-hearted devotion to her own high ends, she will ever find in our legislatures a ready and liberal response. Let us speak out truly our real wants, and we shall surely be heard.

The idea which I have conceived of what a survey of New-York should be, is about the following: Let a base be measured in Western New-York, and made the starting line for a system of primary, secondary and tertiary triangulation, extending towards Pennsylvania and New-England. A connection will be obtained in the Hudson valley with the Coast Survey triangulation, giving the desired verification. Plane tabling should extend, first, over the ground around the cities and large villages, so as soon to furnish good maps of the principal cities and villages, and their vicinities, throughout the State, excepting such as are already covered by the Coast Survey operations. The work should then be extended so as to obtain the elements for complete county maps, to be published in the general order of population of counties, or per square mile. The hydrography of the interior lakes should also enter the scheme. A general State map would follow these. In regard to harbors on the lake coast, it is a question whether their surveys should not be left to the General Government, the data being furnished from the State survey. In point of accuracy and style, the work should not fall essentially below that of the Coast Survey, and might perhaps well be assimilated to the operations for a single section of the coast. These general hints seem sufficient on this point.

The subject now submitted is one which I trust will command the cordial support of this Association; and it is impossible to check the hope that, ere long, New-York, like Massachusetts, will be able to show a fair presentment of her magnificent area. It is not by chance, but by design that this mattor is presented with special reference to this State. Circumstances seem particularly to point to it as the State most needing such a survey, and best able to undertake it. Myself a native and earnest well-wisher of New-York, though now owing a first fealty to all these States united, it will be a grateful imagination to fancy those pictures of her widely diversified and village-dotted territory, which would result from the proposed survey. Flanked by the beautiful Hudson and the sublime Niagara, with her pearl necklace of peaceful lakes and her towering Adirondacks and Catskills, the picture would delight the artist, interest the philosopher, and instruct the statesman.

With the purpose of giving practical form and initial existence to the geographical survey of New-York, and to express what may fairly be presumed to be the sincere desire of this Association, I would now move the following resolutions:

Resolved, That the President of this Association be requested to appoint a committee of five members, to prepare a memorial in the name of this Association, to be addressed to the Governor and Legislature of New-York, urging the speedy commencement of a Geographical Survey of that State, and presenting a matured project of that kind of survey deemed most desirable, with a careful estimate of its cost: this memorial to be duly presented, if possible, before January next.

Resolved, That this committee be requested to consider the general subject of state geographical surveys, and to report thereon at the next annual meeting.

F. MECHANICAL SCIENCE.

II. MANUFACTURES.

1. On the Economical Uses of the Skin of the White Porpoise. By T. S. Hunt, of the Geological Commission of Canada.

Mr. Hunt commenced by a description of the habits and character of this cetacean, the *Delphinus leucos* of Gmelin, which is a native of the Arctic seas, particularly of the Gulf of the St. Lawrence and Hudson's Bay, where it attains a length of from twelve to twenty feet. Its color is of a nearly uniform creamy white. The fisheries of these animals, which are extensive in the lower St. Lawrence, were then described, and the various modes employed for taking the animals.

They have long been valued for their oil, of which they furnish a large quantity; a beluga of twenty feet yielding, in a good season, 150 gallons of superior oil. The general chemical and physical characters of this oil were then remarked upon. It is probably a purer elain than is obtained from any other natural source, and in a cold country is thus of great value. It is now employed exclusively for the lighthouses of the lower St. Lawrence; the board of commissioners having, after a careful trial, given it a preference over all other oils for illuminating purposes.

The skin of the beluga, freed from its epidermis, and a thick mucous layer which underlies it, has been found to be capable of being made into leather of a very superior quality. The process is, in many respects, very different from that which is required for the manufacture of other skins, and is the result of a long series of careful experiments, by Mr. C. H. Tetu, of Rivière Ouelle, who has recently obtained a patent for the invention. The leather, in its ordinary form, has the thickness of sole leather; but its peculiar and valuable property is, the uniformity and closeness of its texture, which enables it to be split into three or four parts, each of which, when dressed, has the smoothness and uniformity of surface which usually belongs to grain leather. Thin sections of it resemble the finest kid, and are employed for the fabrication of gloves. Other important characters

of this leather, are its great strength when compared with calfskin; a peculiar elasticity, which obviates the ordinary tendency to wrinkle and fold; and, to a great degree, imperviousness to water.

Such are the general features of this discovery, which, from the abundance in which the animals are found, promises to be of very high importance in an industrial and commercial point of view.

The new process of M. Tetu has also been successfully applied to skin of the common whale of the gulf, which yields an excellent coarse leather.

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2. The Process of Manufacturing White Zinc Paint, by the New-Jersey Mining Company. By A. C. Farrington, Esq., of Newark, N. J.

The New-Jersey Exploring and Mining Company prepare the white oxide of zinc from the red zinc ores of New-Jersey, in the following manner, at their establishment in Newark:

The crude ore, as obtained from the mine, is stamped and ground so fine that the coarsest grains do not exceed one-eighth of an inch in diameter: it is then mixed with an equal bulk of fine anthracite or dust coal, preparatory to being introduced to the furnace.

The furnace is an oblong muffle, built with fire-bricks, the top of which is a flattened arch not exceeding ten inches in height. The present dimensions of the furnace hearth are seven feet by fourteen. The fire grate is at one end; and the heat is so introduced, that it flows along the top and sides, returning through side-flues under the bottom, and, passing back through a central passage to the rear end of the furnace, makes its exit by descending to an underground flue leading out of the building to the chimney. The charge is introduced through a brick funnel built from the centre of the muffle, and is covered with a tile and with coal dust. The charge is spread and stirred by openings to the muffle each side of the fireplace, and a large one at the opposite end, through which the residuum is raked when the charge is sufficiently worked.

At the end of the furnace opposite the fireplace, there is a flue connected with the interior of the muffle, through which the zinc vapor and carbonic oxide pass. A sheet-iron pipe sixteen inches diameter is adjusted to this flue, having an aperture of about one

hundred inches area for the admission of atmospheric air. This pipe connects with a large one of five feet diameter, that leads into a brick tower twelve feet square and thirty feet in height, having a sheet-iron roof. Near the upper part of this tower, several large pipes pass, for the purpose of conveying air to assist in cooling the heated gases and zinc oxide. From this tower the zinc is conducted through sheet-iron pipes to the receiving rooms, where it is collected in muslin sacks. The oxide of zinc and heated air are drawn from the furnaces by an exhausting blower connected with the sheet-iron pipe, and impelled forward through the tower to the sack receivers. The receivers are sewed together, and suspended in long buildings separate from the furnace building. There are now in use, one, two hundred and sixty feet in length; one, two hundred and forty; and one, eighty: each one is twelve feet wide, and from twelve to sixteen high.

The operation of the work is, that when the heat in the furnace is sufficient to deoxidize or reduce the ore, the vapor of zinc ascends: meeting with a current of atmospheric air, it becomes instantly oxidized, and, passing through the arrangement of pipes named, is caught in the muslin sacks, which are kept distended by the force of the heated air driven into them, but which escapes through the meshes of the cloth without bursting the same. The impure oxide that may contain fine particles of the coal dust mixed with the charge, never passes the brick tower, but falls there, and is taken out at the bottom. The sacks are discharged of white oxide through muslin tubes placed at convenient distances along their bottoms; and when not used, are tied like a flour-bag.

To arrive at the cost of working upon this plan, I transcribed from the superintendent's books the following items of costs for the week ending August 8th, 1851:

69 tons of coal,	at $4,00	$276,00
16½ tons of coal dust,	at 2,50	41,25
28½ tons prepared ore,	at 6,00	151,00
Labor and superintendence		222,00
		$690,25

The product was 38500 pounds of white oxide, showing that about 68 per cent was obtained from the ore used; and the cost of production was a fraction less than one cent and eight mills per pound.

PROCEEDINGS

OF THE

ALBANY MEETING, 1851.

A. HISTORY OF THE MEETING.

The Seventh Meeting of the American Association for the Advancement of Science was held at Albany (New-York), commencing on Monday August 19, and continuing until Saturday August 24. The Standing Committee, as usual, held a meeting on Monday the 26th, for the completion of unfinished business.

The number of members present during the week was greater than at any previous meeting: the list of names registered amounted to 194; and many persons, it is known, omitted their duty in this respect. One hundred and twenty-one new members were elected.

The papers read amounted to 134, classified as shown in the table of contents. It is to be regretted that so many are entered as not received; the authors having failed to send them in, so as to be included in the volume of proceedings.

The general meetings of the Association were held in the rooms of the Albany Academy. The meetings of the sections were held partly in the same building, and partly in the State House.

The Annual Address was delivered by Dr. Bache, the retiring President, in the State House, Thursday evening.

The members of the Association were most liberally entertained on successive evenings by His Excellency Governor Hunt, and by

Messrs. Lansing, Corning, Prentice, and Olcott. By invitation of the City Authorities of Troy, and the Rensselaer Institute of the same place, a meeting was held in this city, after which an entertainment was provided by them. The members of the Association proceeded to Troy in carriages furnished by the corporate authorities of Albany, and, on their way, stopped at the Watervliet Arsenal, by invitation of Colonel Baker.

A number of letters and communications were received from various corporate bodies, offering the use of rooms, collections, etc.; which will be found under a special head.

Among the several invitations in regard to the next place of meeting, the Association accepted that of the City of Cleveland, tendered by its Corporation, through the Mayor, Hon. W. Case. The time selected was the third Wednesday of August (the 18th), 1852.

The expense of publishing the volume of proceedings of the Albany Meeting was assumed by the citizens of Albany, and a sum of money raised sufficiently large for the purpose.

In addition to the papers presented to the Association, various articles of interest were exhibited, as specimens of flax cotton, by Prof. Mitchel; daguerreotypes of the sun during the late eclipse, by D. H. Wells, Esq. Dr. H. Goadby presented microscopic and other objects preserved in his fluids; and Mr. Hotchkiss, a new mode of preserving plants in a herbarium.

The changes in the Constitution passed at the New-Haven Meeting were adopted, with certain modifications, as will be seen under the head of Resolutions of the Association.

The officers elected for the ensuing meeting consist of Prof. B. Peirce of Cambridge, President; Prof. J. D. Dana of New-Haven, General Secretary; Dr. A. L. Elwyn of Philadelphia, Treasurer. The Permanent Secrétary, Prof. S. F. Baird of Washington, holds his office for three years from August 1850.

B. INVITATIONS RECEIVED.

From the Common Council of the City of Albany.

IN COMMON COUNCIL, *August* 4, 1850.

Whereas it has been represented to this Board that a meeting of the American Association for the Advancement of Science is to be held in this city on the third Monday of August instant; and whereas in view of its importance and advantage to the country at large, and in conformity to the established custom of the corporate authorities in other cities where the meetings of the Association have heretofore been held, in proffering their accustomed civilities:

Therefore,

Resolved, That the hospitalities of our city be tendered to the said Association, and that its members be invited to visit the public institutions and the Rural Cemetery; and that the Mayor, with the Committee on Academies and Schools, be requested to make the necessary arrangements for carrying the same into effect.

Adopted. (A true copy.) THOMAS B. MORROW, *Clerk*.

From the Regents of the University of the State of New-York.

At a Meeting of the Regents of the University of the State of New-York, held June 25, 1851:

The Regents having been officially informed that the Annual Meeting of the American Association for the Advancement of Science is appointed to be held in this city during the month of August next:

Resolved, That the Regents hereby invite the members of the Association to visit the State Library and the State Cabinet of Natural History, and to avail themselves of the privileges granted to the visitors of those collections.

Resolved, That a copy of the above be sent to the Local Secretary of the Association. (A true extract from the minutes.)

T. ROMEYN BECK, *Secretary*.

From the Albany Institute.

At a Meeting of the Albany Institute, held June 19th, 1851, the following resolution was passed:

Resolved, That the use of the rooms, library and collections of the Institute be offered to the American Association for the Advancement of Science, during its coming session in this city. GEORGE H. COOK, *Rec. Secretary*.

From the Albany Academy.

ALBANY ACADEMY, *June* 16, 1851.

G. R. PERKINS, ESQ.

DEAR SIR : At a Meeting of the Trustees of the Albany Academy, the following preamble and resolutions were unanimously adopted :

The Trustees of the Albany Academy having been informed that a meeting of the American Association for the Advancement of Science had been appointed to be held in this city in August 1851 :

Resolved, That the use of the rooms of the Academy be tendered to said Association for its meetings.

Resolved, That a copy of this resolution be forwarded to the Local Secretary of the Association. (True copy.) WILLIAM H. CAMPBELL, *Clerk*.

From the Albany Female Academy.

THE Trustees of the Albany Female Academy learn with pleasure that a meeting of the American Association for the Advancement of Science will be held in this city in August next; and in tendering the members of said Association a welcome, we would also tender them the use of our rooms, and free access to our library.

ALBANY, *July* 3, 1851. By Order of the Board.

W. L. MARCY, *President*.
L. S. PARSONS, *Secretary*.

From the Albany Medical College.

THE Trustees of the Albany Medical College, having learned that the Annual Meeting of the American Association for the Advancement of Science is to be held in this city during the present month, would respectfully invite the members of the same to visit their institution : they also tender them the use of their rooms, for any of their general or section meetings. IRA HARRIS, *President*.

ALBANY, *August* 1, 1851.

From the State Normal School.

ALBANY, *June* 17, 1851.

G. R. PERKINS, ESQ.

DEAR SIR : At a Meeting of the Executive Committee of the State Normal School, held to-day, the following preamble and resolutions were unanimously adopted :

The Executive Committee of the State Normal School, being informed that the Annual Meeting of the American Association for the Advancement of Science will be held in this city in August next,

Resolved, That the use of the rooms in the State Normal School be tendered to said Association, for its meetings.

Resolved, That a copy of this resolution be forwarded to the Local Secretary of the Association. (True copy.) WILLIAM H. CAMPBELL, *Secretary*.

From the Albany Young Men's Association.

ROOMS OF THE ASSOCIATION, ALBANY, *June* 14, 1851.

WHEREAS the American Association for the Advancement of Science have favored this city as the place of their meeting in August next:

Resolved, That we extend to the members of said Association our friendly salutations, and a cordial welcome to our city.

Resolved, That the free use of our reading rooms, lecture room and library, be, and the same is hereby tendered to the members of the said Association, during their stay in Albany.

Resolved, That this Association, collectively and as individuals, will take pride in rendering the visit of the American Association among us agreeable to its distinguished members, so that it may be a pleasure to them both to remember and to renew it.

Resolved, That a copy of these resolutions, signed by the President, and duly attested by the Recording Secretary, be forwarded to the said Association.

(A true copy from the minutes.) J. I. JOHNSON, *President.*

M. C. G. NICHOLS, *Recording Secretary.*

From the Rensselaer Polytechnic Institute.

Resolved by the Trustees of the Rensselaer Polytechnic Institute, That their best wishes, and the hospitalities of the Corporation be tendered to the American Association; and that its members be invited to visit the rooms of the Institution, at such time during their session at Albany as may best suit their convenience.

Resolved, That Professor GREENE be requested to communicate the foregoing resolution to the American Association, as early as may be expedient, at their present meeting. STEPHEN WICKES, *Secretary of the*

TROY, *August* 18, 1851. *Corporation of the Rens. Pol. Institute.*

From the Rensselaer Polytechnic Institute.

TO THE STANDING COMMITTEE OF THE AMERICAN ASSOCIATION.

THE Trustees of the Rensselaer Polytechnic Institute, hearing with great gratification that it is proposed by the American Association to hold one of its morning sessions at Troy, on the occasion of its intended excursion on Thursday:

Therefore,

Resolved, That the physical lecture room of the Institute, with such other rooms as may be needed, be tendered for the use of the Association in holding the meeting before referred to.

I. M'CONIHE, } *Prudential Com.*
T. C. BRINSMADE, }

TROY, *August* 19, 1851.

From the Common Council of the City of Cleveland.

MAYOR'S OFFICE, CLEVELAND, *August* 18, 1851.

SIR: By a unanimous resolution of the City Council of Cleveland, I am requested to invite the American Association for the Advancement of Science, over which you preside, to hold their next annual meeting at Cleveland; and to assure you that a place for meeting will be furnished, and the proceedings of the Convention cheerfully published at the expense of the City.

Allow me, sir, to hope that it may meet the views of the Association to accept this invitation.

I remain yours truly,

WILLIAM CASE, *Mayor.*

To Prof. AGASSIZ, *President of Am. Association.*

C. COMMITTEES FROM WHICH REPORTS WERE DUE*.

Committee on the Paper of Prof. MITCHEL. Reported and discharged.

Committee on Prof. COAKLEY's Elliptic Tables of Neptune. Reported and discharged.

Committee to examine the Experiments of Prof. JOHNSON on Coals. Discharged.

Committee on Physical Constants. Discharged.

Committee on Annual Assessments and Tickets. Continued.

Committee on a Change in the Constitution providing for honorary members. Discharged.

Committee on the Communication of Lieut. MAURY on Winds and Currents. Discharged.

Committee on the Prime Meridian. Discharged.

Committee in relation to the United States Coast Survey. Continued.

Committee on a Uniform Standard of Weights and Measures. Continued.

Committee to memorialize State Governments upon the importance of commencing or continuing Geological Surveys. Continued.

Committee to memorialize the Legislature of Ohio, on the subject of a Geological Exploration of the State. Continued.

Committee to memorialize the State of Missouri on the same subject. Continued.

Committee for memorializing Congress in relation to Scientific Explorations. Continued.

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* The names of the committees will be found in the beginning of the volume.

D. NEW COMMITTEES APPOINTED.

1. To examine Mr. LYMAN's Telescope; the Stand for Mr. SIMMONS's Telescope; and the Mountain Barometer of Mr. ANDREWS. Reported and discharged.
2. Relative to Dr. HOUGH's Communication in respect to reducing and publishing Meteorological Observations in the State of New-York. Reported and discharged.
3. To examine the Microscope and Lenses brought to the Meeting of the Association by Mr. SPENCER. Reported and discharged.
4. To memorialize Congress for an Appropriation to enable Prof. MITCHEL to perfect and apply his New Astronomical Apparatus.
5. To arrange the Details of a System of Combined Meteorological Observations for North America. Reported and continued.
6. To digest a plan of reducing the observations made under the direction of the Regents of the University of New-York from 1825 to 1850, with reference to their publication, and to decide upon the stations which shall be included in this reduction.
7. To memorialize the State of New-York and others, in regard to geographical surveys.

E. REPORTS OF COMMITTEES.

REPORT OF THE COMMITTEE ON PROF. MITCHEL'S NEW METHOD OF OBSERVING AND RECORDING RIGHT ASCENSION AND NORTH POLAR DISTANCES.

Prof. PEIRCE presented the conclusion of the report on the observation of right ascension and north polar distances. The committee were very much in favor of the discoveries made by Prof. MITCHEL. They recommended that a committee be appointed to memorialize Congress to aid the professor in bringing his investigations to a close.

Prof. MITCHEL then explained the old methods of making these observations, and compared them with his new method. He showed that by means of a magnetic key, when the star was bisected, the

record was made at once. He had encountered many difficulties in his examinations : sometimes so greatly did they increase around him, that they taxed his perseverance to the utmost. When he got to measuring very minute arcs, he had met with great sources of error, which he had eventually overcome. He had not the means to pursue the investigation farther, so that he had been obliged to leave his apparatus in an imperfect state.

Since the New-Haven meeting, he had commenced a series of observations on the sun's diameter, which he compared with the observations in the Nautical Almanac. He showed the minute power of the instrument which he had contrived. Having found that his micrometer was imperfect, after reflection he had come to the conclusion that it might be made more perfect. He then explained how he had observed the declination of the various stars as recorded. No hand in the world had sufficient steadiness to make such minute marks as the exquisite marks made by the instrument which he had used. When he discovered how perfect was the record of the several stars which he had observed, he never had had such feelings in his whole life. He named the stars on which he had made observations. When he had found out to what the discrepancies were owing, he went back with the micrometer and examined them still farther, until he had perfectly satisfied himself where these differences occurred.

Report of the Committee on Prof. Coakley's Elliptic Tables of Neptune.

Lieut. Davis, from the Committee on Prof. Coakley's Tables of Neptune, made, by its direction, the following report:

The committee, after deliberation, has decided to report against the publication of these tables, not because they are in the slightest degree deficient in accuracy, but because it is of the opinion that the publication of purely elliptic tables of a planet, unaccompanied by tables of the perturbations, would be a retrograde step in astronomy.

Report of the Committee on Dr. Hough's Communication respecting the Meteorological Observations made in the State of New-York.

The Committee to whom was referred the communication of Dr. Hough, relative to reducing and publishing the Meteorological Observations made in the State of New-York from 1825 to 1850, report the following resolutions:

Resolved, That in the judgment of this committee, it is important that the results of the meteorological observations made in the State of New-York during the last twenty-four years, under the directions of the Regents of the University, be published.

Resolved, That a special committee of three be appointed to digest a plan of reduction of the observations, with reference to their publication, and to decide upon the stations which shall be included in this reduction.

Resolved, That Dr. Beck and Professors Guyot and Loomis be that committee.

Report of the Committee on Microscopes, on the Lenses exhibited to the Association by Mr. Spencer.

The Committee on Microscopes have the honor to report, that they have carefully examined several sets of microscopic objectives, recently manufactured by Mr. Charles A. Spencer, of Canastota, New-York; and that, after numerous trials with the most difficult test objects known, they are unanimously of opinion that these lenses are of unrivalled excellence.

The perfection of these glasses was shown by their admirable defining power, their unprecedented largeness of aperture still preserving good working distance, and by freedom from the ordinary defects of lenses. The committee believe it unnecessary to report in detail the different experiments made, but confine themselves to the statement, that after numerous trials by all the modes and tests which have been repeatedly employed by different members of the committee in examining many of the best foreign lenses, they have arrived at results with Mr. Spencer's objectives which they believe have never hitherto been obtained by any microscopes in existence. The low powers, as well as the high ones, excited their admiration,

readily and beautifully resolving test objects hitherto considered entirely beyond the reach of glasses of corresponding focal distance.

As every improvement in the microscope has a direct and most important influence on the progress of scientific research, the committee believe that they cannot express in too strong terms their admiration of the results obtained by the unaided efforts of Mr. Spencer; and however reluctant to appear in a boastful attitude, they believe it would be an act of injustice not to state their sincere conviction that Spencer's objectives are now the best in the world.

Report of the Committee on Meteorology, on the Arrangement of a System of Combined Meteorological Observations for North America.

The Committee on Meteorology, to whom was referred by the Association the "Proposition for extending the system of meteorological observations now in operation under the direction of the Smithsonian Institution," respectfully present their report, and ask of the Standing Committee their consideration of it, and of the resolutions appended to it, so that they may be presented, if approved, to the Association at the present meeting.

It is not necessary, at the present day, to go into any argument on the importance of such observations. Wherever civilization extends, their value is recognized, and they are sustained by private and public exertions. At different times, systems of observations have been organized by different governments and societies of the Old World, for determining the general and particular questions which occur; and in our country, the General Government, and several of the State governments, as New-York, Pennsylvania, Massachusetts, have kept up, for a limited time, several series of meteorological observations, from which results of high importance have been derived.

Recently the British Government have determined to maintain the Magnetical and Meteorological Observatory at Toronto, where full observations are made with instruments registering by photographic methods. Our own Government still keeps up the observations at the military posts, under the enlightened supervision of the

Surgeon General of the Army. The Treasury Department has, not long since, expressed the opinion that the keepers of light-houses should be sufficiently well instructed to make such observations. The Navy Department fosters the meteorological observations under the direction of the Smithsonian Institution. The Hudson's Bay Company have recently consented, on application of the Association, to establish observations at such of their posts as might seem desirable to the Association. The States of New-York and of Massachusetts have renewed their action in the matter. There is a great desire to profit by these very favorable circumstances of our country, and of the present day, to organize a system, which shall connect all these efforts, otherwise isolated, and to derive from these and from similar ones the means of advancing the knowledge of the meteorology of North America.

We expect to derive from systematic observations, extended over as much of our continent as is accessible to us, at stations selected in reference to the problems to be made out, a thorough knowledge of our climate in all its relations, and of its variations in the same and in different localities. The mean temperature of points is to be determined with carefully verified instruments, similar to each other, similarly placed, and observed under the same rules and conditions: the lines of equal mean temperature will result, and the variations at different seasons will be shown. The limits of vegetation will be found, and the areas of climate adapted to the cereals. The parallels within which wheat, indian corn, etc. may be profitably cultivated, and which present results so different from those found to exist in the eastern continent in Asia, will be determined accurately. The degree of dryness and moisture will be ascertained; the amount of rain, and the amount of evaporation : questions not only bearing upon the health and comfort of man, but upon his attempts to facilitate communication by canals and the improvement of rivers, and upon the means of avoiding or controlling floods and freshets. The number of days of rain, the number of clear and cloudy days, and the amount of loss of the sun's effect by cloudiness, will be determined; the direction and force of the wind, and the systems of winds prevailing in different parts of the continent, and in the different seasons of the year. The mean pressure of the air and its variations will be seen, as shown by the barometer; from which important data in regard to relative heights of points may be obtained, giving the general topographical features of our extended country, and

serving as a reconnoisance in more distant parts of it for railroads or common roads which may be proposed. The progress of waves of pressure, either connected with storms or with the ordinary fluctuations of the atmosphere, will be ascertained. All periodical phenomena will be studied in connection with these observations; the flowering of plants and trees, the ripening of grains and of fruits, the migrations of animals. The frequence and intensity of the aurora borealis will be determined; and its singular variations in passing from north to south, and east to west, on our continent, will be studied. The direction of the motion, the frequency, the intensity, and other circumstances actuating our thunderstorms, will be ascertained. From the observations will result the law of storms in its full development; and its application to all parts of the continent, or limitation to particular portions, will be entirely ascertained; an application so important to the farmer and navigator, so interesting to the man of science, and so desirable to be known by every one who travels on any of our lakes or rivers, or along our extensive and sometimes stormy coasts. The lines of our telegraphs will be rendered available for observations on this subject, more complete than any which have been hitherto practicable; and while they enable us to determine the laws of storms, will also furnish the means of giving notice of their progress, and then of anticipating their approach.

The diseases incident to different climates, the phenomena of malaria, the progress and laws of epidemics, may be studied in connection with the periodical phenomena from carefully collected statistics.

A contribution to ethnology may be the statistics of the numerical decrease of the Indian races, to the interest of which many minds in this country are fully alive.

These are only a portion of the results which may be expected from a wide-spread and well-organized system of meteorological observations.

We would propose, therefore, to establish at once, in addition to those which now exist, fifty meteorological stations in the positions named below; to supply the primary stations with a full set of instruments, carefully compared and of uniform construction, namely, a thermometer, barometer, hygrometer, rain and snow gage, and wind vane; to cause hourly observations to be made at six or eight stations, and observations three times a day at all others, according to the same system.

The following list includes stations already occupied, which are, however, marked to distinguish them from those which it is proposed to establish :

LIST OF THE METEOROLOGICAL STATIONS TO BE ESTABLISHED.

SUB-TROPICAL ZONE. Key West, Florida; Point Isabel, Rio del Norte; New-Orleans, Louisiana.

WARM TEMPERATE ZONE. *A.* St. Augustine, Florida; Tuscaloosa, Alabama; Vicksburgh, Mississippi; Fredericksburg, Texas; El Paso, New Mexico; San Diego, California. *B.* Chapel Hill, Carolina; Knoxville, East Tennessee; Nashville, West Tennessee; Fort Atkinson, Indian Territory: Fort Washita, Indian Territory; Santa Fe, New Mexico.

MIDDLE TEMPERATE ZONE. *A. X.* Washington, D. C.; Lexington, Virginia; Fort Leavenworth, Indian Territory; Bent's Fort, do. *B. X.* New-York City, New-York; a central point, Pennsylvania; Steubenville, Ohio; Columbus, do.; Indianapolis, Indiana; Springfield, Illinois; Bloomington, Iowa.

MIDDLE TEMPERATE ZONE (continued). Fort Kearney, M. T.; Fort Laramie, M. T.; Salt Lake, Utah: Fort Hall, Oregon; Nueva Helvetia, California; San Francisco, do.

COLD TEMPERATE ZONE. Bowdoin College, Maine; Dartmouth College, New-Hampshire; Burlington, Vermont; Kingston, Canada; Manitoulin Island, or Bruce Mines, Canada; Lansing, Michigan; Milwaukee, Wisconsin; Fort Gaines, Minnesota; Fort St. Pierre, Minnesota Territory; Fort McKenzie, do.; Fort Kootanie, Oregon; Fort Walla Walla, do.; Fort Vancouver, do.; three new light-houses along the coast from Oregon to Point Conception, California.

RESOLUTIONS.

Resolved, That the Committee on Meteorology recommend to the American Association for the Advancement of Science, the appointment of a committee to memorialize Congress in regard to the *immediate* extension of the system of meteorological observations now making in the United States, under the direction of the Smithsonian Institution.

Resolved, That this committee be authorized and directed to request the Secretary of the Treasury to provide for the making of meteorological observations, according to the directions of the Smithsonian Institution, by the keepers of the light-houses which are to be established at points on the Western Coast of the United States, named in the reports of the committee on meteorology.

Resolved, That the same committee be requested to address the Surgeon General of the United States in reference to the co-operation of this department in the same system, and to suggest the locations named in the report of the committee on meteorology as those where the observations should be made.

Resolved, That the same committee be requested to memorialize the Canadian Government, and the several legislatures of the States of our Union, asking their co-operation in the foregoing system of observations.

Resolved, That the same committee be requested to inform the Hudson's Bay Company of the steps which have been taken to carry into effect the system referred to in the memorial addressed to them by this Association, at the last annual meeting.

F. RESOLUTIONS AND ACTS OF THE ASSOCIATION.

Resolved, That no papers be accepted by the Association, unless their titles be registered before Thursday morning (unless by vote of the standing committee).

Resolved, That the standing committees of the sections be requested, before the close of the meeting, to present to the Permanent Secretary a list of the papers which have been read in the sections, and which they desire to have published.

Resolved, That hereafter all members of this Association are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Resolved, That the foregoing resolution form part of the Circular.

Resolved, That, considering the circumstances in which they were placed, the members of this Association do entirely approve of the action of the standing committee of this year.

Resolved, That the annual fee of membership be $2,00; and that payment of an additional dollar entitle a member to a copy of the Proceedings of the Meeting.

Resolved, That all members of the Association who have not paid their dues, after the issue of two circulars at intervals of three months, notifying them of that fact, be stricken from the roll by the Permanent Secretary.

Resolved, That the President of this Association be requested to appoint a committee of seven members, to prepare a memorial in the name of this Association, to be addressed to the Governor and Legislature of New-York, urging the speedy commencement of a geographical survey of that State, and presenting a matured project of that kind of survey deemed most desirable, with a careful estimate of its cost : this memorial to be duly presented, if possible, before January next.

Resolved, That this committee be requested to consider the general subject of State Geographical Surveys, and to report thereon at the next annual meeting.

Resolved, That the American Association for the Advancement of Science feel it due to the eminent scientific labors of the late Samuel G. Morton, of Philadelphia, to express their deep regret at the loss which science has sustained in the death of their distinguished colleague.

Resolved, That a copy of these resolutions be transmitted by the President and Secretary to the family of the late S. G. Morton.

Resolved, That the thanks of the American Association for the Advancement of Science be returned to the Mayor and Corporation of the City of Albany, for the measures taken by them to extend the hospitalities of their city to the members, and to enable them to visit its beautiful environs and the neighboring city of Troy.

Resolved, That the thanks of this Association be tendered to the Local Committee, for the ample accommodation and complete arrangements which have so much promoted the business which has come before this body.

Resolved, That the thanks of this Association are hereby presented to the Trustees of the Capitol, to the Trustees of the Albany Female Academy, to the Executive Committee of the State Normal School, for accommodations tendered this body during its sitting.

Resolved, That the thanks of the Association be presented to the Governor of the State, and to the other gentlemen who have entertained the Association, for their kind hospitality and attention to the members and their families; and also to Col. Baker, of the U. S. Arsenal, for his reception of the Association at the Arsenal; and likewise to the Rensselaer Polytechnic Institution.

Resolved, That the thanks of the Association be presented to the Albany Academy, for the facility afforded in its rooms for their meeting; and also to the Albany Institute, for the use of its library and collections.

Resolved, That the thanks of the American Association be presented to the Trustees and Faculty of the Albany Medical College, for the kind offer of the use of their rooms for the purposes of meeting, and also for the invitation to visit the museum of the same.

Resolved, That the thanks of this Association be likewise tendered the members of the Young Men's Association, for the kind offer of the use of their rooms to the members of this body.

Resolved, That the thanks of the Association be returned to the Superintendent of the Western Railroad, and of the roads from Cincinnati to Cleveland and Sandusky, and of the Utica and Schenectady Railroad, for the facilities offered to members in their passage to and from the Albany meeting.

Resolved, That the thanks of the Association be presented to the Regents of the University of the State of New-York, for their attentions to the Association while in this city.

Resolved, That the thanks of the Association be presented to those gentlemen of Albany, who have subscribed towards publishing the Proceedings of the Association; and that those giving $5,00 and upwards, receive a copy of the Proceedings.

Resolved, That the Section on Meteorology approves entirely the mode of testing and graduating the thermometer, and the arrangement and construction of the barometer, and other meteorological instruments adopted by the Smithsonian Institution and by the States of New-York and Massachusetts, and now explained by Professor GUYOT.

Resolved, That the Permanent Secretary be directed to collect all the actual rules, regulations and resolutions of the Association, and publish them in the next volume of Proceedings as an appendix to the Constitution.

Resolved, That a committee be appointed, to represent to the Secretary of the Interior the importance of presenting, in Major EMORY's Report on the United States and Mexican Boundary, the zoological results obtained by Dr. LE CONTE and others in the same section of country. Said committee to consist of Prof. AGASSIZ, Dr. TORREY, Prof. DANA, Dr. KIRTLAND, Prof. BAIRD.

Resolved, That the Committee on Meteorological Observations be continued, to act in reference to the extension of the system of meteorological observations now in operation under the direction of the Smithsonian Institution.

WHEREAS the provision of the Constitution appears to be indefinite in regard to the term of service of the chairmen and secretaries of the Sections :

Resolved, That the sections be requested to direct the chairmen of their several standing committees to attend to the current business of the section, and to appoint a chairman for each day of the meeting.

Resolved, That the sections be requested to appoint a secretary for the period of the meeting, whose duty it shall be to furnish to the Permanent Secretary, for publication, a full report of all proceedings and discussions.

Resolved, That the following resolutions be presented to the Association at the opening of the Cleveland Meeting, for adoption:

1. That all papers, either at the general or in the several sectional meetings, shall be read in the order in which they are entered upon the books of the Association; except that those which may be entered by a member of the standing committee of the Association, shall be liable to postponement by the standing committees of the sections.

If this regulation should be adopted by the Association, members will recognize the expediency of entering the titles of their communications at as early a date as possible.

2. That if any communication should not be ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

3. That no exchanges shall be made between members, without authority of the respective standing committees.

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CORRECTIONS.

Page 74. Art. 12, "Relation of the chemical constitution of bodies to light," should be entered as *not received*, and the entire article regarded as cancelled; the same having been reprinted from a mere rude newspaper report, unrevised by the author, and subsequently disavowed.

Page 207. Since art. 7, "Solidification of the rocks of the Florida reefs, and the sources of lime in the growth of corals," was printed, Prof. J. D. DANA has stated to the author, that "the remarks cited in the 33d line of page 213, from his Geological Report, were not intended to convey the idea that corals flourish best in the vicinity of fresh waters."

Page 240. The formula in line 33 should be

$$HO,SiO_3,Cr_2O_3,Fe_2O_3.$$